INQUIRY

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CHANCE IN SOCIAL AFFAIRS*

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The Norwegian Law of Courts § 51 states: "Well in advance of each session of the circuit court, hearing criminal cases, the chief judge, or another judge authorized by him, or the court's clerk shall, in the presence of a legal witness, *by drawing lots,* select fourteen jury-members and two alternates." This is one among a great many cases where society has institutionalized a recognized chance device as the proper mode of reaching a decision with social implications.

More than 150 years ago the English minister-philosopher Paley advanced the embryo of a general theory about the social functions of such chance decisions: "In a great variety of cases, and of cases comprehending numerous subdivisions, it appears, for many reasons, to be better that events rise up by *chance,* or, more properly speaking, with the appearance of chance, than according to any observable rule whatever. This is not seldom the case even in human arrangements. Each person's place and precedency in a public meeting may be determined by lot. Work and labour may be *allotted.* Tasks and burdens may be allotted. Military service and station may be *allotted.* The distribution of provision may be made by *lot,* as it is in a sailor's mess; in some cases also the distribution of favours may be made by *lot.* In all these cases it seems to be acknowledged that there are advantages in permitting events to chance superior to those which would or could arise from regulation. In all these cases also, though events rise up in the way of chance, it is by appointment that they do so".[1] And he goes on to say that " . . . even the acquirability of civil advantages, ought perhaps in a considerable degree to lie at the mercy of chance. Some would have all the virtuous rich, or, at least, removed from the evils of poverty, without perceiving, I suppose, the consequence, that all the poor must be wicked.

And how such a society could be kept in subjection to governments, has not been shown, . . ."[2]

Paley refers to chance devices that "by appointment" are used to make decisions. The chance element is, as in the selection of juries, a *manifest* characteristic of certain institutionalized types of decisions. If we leave our own rationalistic culture, or those areas where we are the most deliberate and utilitarian, we come across another vast class of decisions that *are* random, but without being recognised as such. The chanciness of these decisions is *latent*.

One interesting step towards extending the concept of the chance device in social decisions has been taken by O. K. Moore in his analysis of hunting-magic among the Naskapi Indians in Labrador. The magical practice consists of heating bones of animals over hot coals, usually the shoulder-blade of the caribou or a bone of the kind of animal which the Naskapi are about to hunt. When heated, the bone cracks. It is then fitted into a wooden handle, and while held in specified ways, the cracks of the bone are read so as to give directions for the hunt. "An impersonal device of the kind used by the Naskapi might be characterized as a crude "chance-like" instrument. It seems that the use of such a device would make it more difficult to anticipate their behaviour than would otherwise be the case".[3]

The practice serves to increase the likelihood of successful hunting under conditions where game is scarce and where the animals tend to learn from previous experience with hunters. The Naskapi are, however, quite unaware of the randomness resulting from these decisions. They believe that they are seeking, and getting, guidance from the supernatural, i. e. that the decision is "systematic". This seems to be generally true for all similar practices among primitive people. The conscious *notion* of chance appears to be a late comer among the basic conceptual tools by which man gains mastery over his world of action and perception. Sigerist states categorically that primitive men always deny chance or randomness in the occurrence of disease.[4] Illness is always precipitated either by natural causes or by the sick or somebody else's actions. Similarly, death is usually not explained as due to chance.[5] On the whole, it seems to be the consensus of anthropological opinion that misfortunes and other events, are never explained by reference to a chance concept. On the contrary, primitive man's systems of belief usually give evidence of a very vigorous denial of chance and uncertainty, and a similarly desperate affirmation of their capacity to master the world by secular skills or by magic.[6]

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It has already been suggested that there is a need to distinguish between what we, as scientific observers, would characterize as chance elements in social life, and what any particular individual or social group happens to classify as a chance event. It may actually be useful to distinguish between three sociological types of chance phenomena: random responses precipitated by ignorance, chance devices, and chance theories.

Ignorance

Several years ago Moore and Tumin published a paper on the functions of *Ignorance* in social affairs. Some of their conclusions merit serious consideration in a context of chance decisions, although the writers themselves were unconcerned with this aspect of their problem: "The central theory of this paper holds that, quite apart from the role of ultimate values and the attitudes relative to them, perfect knowledge is itself impossible, and an inherently impossible basis of social action and social relations. Put conversely, ignorance is both inescapable and an intrinsic element in social organization generally, although there are marked differences in the specific forms, degrees, and functions of ignorance in known social organizations."[7] And they go on to describe some possible functions of ignorance in modern western societies: "The function of ignorance that is most obvious, particularly to the cynical, is its role in preserving social differentials."[8] "Ignorance operates to maintain smooth social relations by preventing jealousy and internal dissension where differential rewards to approximate status equals are not based upon uniformly known and accepted criteria."[9] "The success of military or law-enforcement undertaking, and the security of its participants, may depend upon the element of *surprise*."[10] "Another way in which ignorance serves to protect the traditional normative structure is through reinforcing the assumption that deviation from the rules is statistically insignificant."[11]

Whenever there is ignorance, secrecy, or deception about an event, the actor's response and decision relative to that event, must in one sense be random. He does not fully know what he is doing. The influence of ignorance and secrecy upon his *perception* of order or randomness depends upon the reason why he is kept ignorant. If his ignorance is related to the unacceptably random nature of the events (like unequal rewards in an office organization) it may give him more sense of orderliness than is warranted. If his ignorance is due to a need to hide a system in

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certain events, he will perceive more randomness in the events than there "is". One might venture the hypothesis that ignorance of randomness is most likely to be kept up within co-operative social bonds; while ignorance of systematic responses is most likely to be kept up in antagonistic social relations and in conflict behavior.

Chance Theories

By *"Chance theories"* is meant everything from the calculus of probability to everyday attempts to explain events by reference to luck, accidents, fortune, or the like, with the emphasis upon the latter "folk theories". The functional significance of such theories has been discussed by Merton: "In sociological terms, the doctrine of luck as expounded by the successful serves the dual function of explaining the frequent discrepancy between merit and reward while keeping immune from criticism a social structure which allows this discrepancy to become frequent. For if success is primarily a matter of luck, if it is just in the blind nature of things, if it bloweth where it listeth and thou canst not tell whence it cometh, or whither it goeth, then surely it is beyond control and will occur in the same measure *whatever the social structure.*

For the unsuccessful and particularly for those among the unsuccessful who find little reward for their merit and their effort, the doctrine of luck serves the psychological function of enabling them to preserve their self-esteem in the face of failure. It may also entail the dysfunction of curbing motivation for sustained endeavour . . .

This orientation toward chance and risk-taking accentuated by the strains of frustrated aspirations may help explain the marked interest in gambling — an institutionally proscribed, or at best, permissive rather than preferred or proscribed mode of activity — within certain social strata".[12]

The idea of Fortune as a category of events appeared in Aristotle's works. But, beginning with him, the concept of Fortune has always been very close to its opposite pole, Fate.[13] This ambivalence reappears in people's everyday philosophies relative to a large number of social situations; what seems to be determined by chance is at the same time decided in advance by fate. The ideology of romantic love gives an explanation of the decision on whom to love and/ or marry, which has a strong element of chance, with overtones of fate. The English term "to fall in love" carries with it associations of randomness and chance. The more extreme cult of romantic love, is full of expressions like "love at first sight", "love in spite of reasons". A Norwegian proverb says

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that "love may fall upon a dirt as soon as upon a lily". One implication seems to be that love is unpredictable, irrational and divorced from other systems of social interaction.

The notion of "the happy accident" in science is another case in point, irrespective of whether this is a correct explanation or not, of how inventions and discoveries actually are made. A more special case is the institution of "free association" in psychoanalysis. The norm directed to the patient is that he should permit "random" thoughts to become conscious and verbalize them, while the explanation applied by the therapist is a systematic one. We may look upon the recent method of "brainstorm" as an extension of free association on the psychoanalyst's couch.

Chance Devices

The objective chance device is a physical test to the outcomes of which are assigned values in advance. Man has under a great variety of circumstances chosen to delegate decisions to such tests — ranging from matters of life and death to mere trivia.

Games of Chance are the prototype of chance mechanisms. The mathematical theory of chance and probability itself has developed out of experiences with lot-drawing, dice-throwing, card-playing and roulette. Games of chance are found as far back as we can go in the history of Western society, and among a large number of primitive societies. On the basis of a threefold division of games; games of chance, of skill, and of strategy, it has been found that the relative frequency of occurrence of these three kinds of games varies with geographical and political factors. In 51 cultures represented in the Cross-Cultural files of Yale University, which were judged to be adequately covered on the topic of games, 20 had games of chance.[14]

Games of chance belong in a specific sheltered area of existence, the sphere of "play" as against the serious business of living. It is an activity of Homo Ludens.[15] But games of chance interest us here, not because they represent a retreat from the serious business of living, but because the games effect settlements that impinge upon "serious" social reality, e. g. by redistributing money, goods, prestige, the benevolence of the gods, future prospects in love and business, the joys of victory, and frustrations of defeat.[16]

Trial by ordeal is another institutionalized chance device. Vinogradoff divides the ordeals into three types: Struggle, appeals to chance, and

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appeals to miracles against a human presumption.[17] The distinction between the two last ones is probably one of degree, the odds being very heavily stacked against one of the parties in the latter case, leaving less scope to the operation of chance factors.

Concerning those ordeals which apparently appeal to chance, one recent writer has claimed that they were set up so as to give the innocent, and therefore psychologically relaxed, a strong advantage over the guilty and psychologically tense.[18] There is some slight plausibility for this view in the bread-eating ordeal and a few others. On the whole, this theory, which likens the old Germanic ordeals to modern legal-psychiatric observations, seems rather far-fetched. Most writers on the subject appear unconcerned with the degree of system and uncertainty in trials by ordeal, which makes it hard to determine whether the ordeal functioned as a chance device or not.

Trials by ordeal are best known from mediaeval Germanic law.[19] From early mediaeval times such trials were occasionally fought by the church;[20] but the last remnant of the English statute authorizing trial by battle was not repealed until 1819. Trials by ordeal are also well-known from old Indian laws.[21] Very few examples are known from the other great civilizations of antiquity, the Babylonian, the Judaic and the Greek. The Bible does, however, in a few places, refer to trial by lot-ordeal, the one ordeal most obviously based upon a chance device.[22] Finally, trial by ordeal is known from many primitive legal systems.[23]

An omen is not a decision on how to act, but a way of deciding on what to expect. The prediction may or may not have consequences for action. It may become a self-fulfilling prophecy, or a self-defeating one, or it may result in a conviction with more diffuse behavioral implications, e. g. greater or less self-assurance in general. Unlike the known ordeals, which fall in relatively few distinctive types, there exists an indefinite variety of methods of *divination* and tests from which omens are drawn. Frazer's "Golden Bough" contains a fabulous wealth of material on omens and divination.[24]

We find here two groups of tests, which differ in one apparently significant respect. The first group of tests is made up of games of skill, the outcome of which depends very largely upon physical prowess and the will of the actor. The outcome of the second group of tests lies at the mercy of chance. The importance of this distinction may nevertheless be doubted in this particular context since the *relationships* between the tests of skill and the predicted events, unlike relations between examina or I. Q. tests, and later occupational performances, seem to be

random. In other words, the pre-established codes of interpretation, by which numerous outcomes of the tests are assigned meanings, are so constructed that they closely parallel modern lists of random numbers, transform systematic responses into a random distribution of values. Thus, any coincidence between test result and individual fate in real life would appear to imply super-natural interference, either in the test or in the life of the person. It should be noted, however, that in tests of skill, the person may express his motivation relative to the predicted event, and thus render the test into a rational symptom of the predicted state of affairs.

FUNCTIONS

Communication with the supernational

All known cultures, except possibly the emergent communist civilizations, assume the existence and significance of a supernatural world. To communicate with this vital sphere of existence presents grave problems, however. Man may speak to the supernatural, but will the supernatural answer? Or rather, how is it possible to obtain "answers" from the other world which do not obviously originate within everyday profane existence? Natural events can, of course, be viewed as symptomatic of states in the other world and of man's relationship to the other world. But there are two ways in which men may obtain answers that are inexplicable in terms of ordinary experience, and therefore achieve a special status relative to the supernatural: through dreams and chance devices.

Chance devices, as we know them from games, ordeals, omens, oracles, divination and other chance contraptions, have in common that the outcome, to varying degrees, is unpredictable on the basis of the natural or moral qualities of the person with regard to whom a decision is being reached. It is, furthermore, unpredictable on the basis of the known characteristics, interests, motives, abilities, etc. of the social agent administering the test. The answer from the chance device is unpolluted by any carrier of the message, a priest, a prophet or a judge. Answers given by chance devices cannot meaningfully be ascribed to a human choice. Neither can they be ascribed to the laws of nature, since the test is set up precisely in such a way as to make the outcome uncertain on the basis of known laws of nature. It is not unnatural to conclude that what is not decided by men or by nature, must be decided by the gods.

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The close relationship between games of chance, divination and man's communication with the supernatural world was pointed out by the outstanding nineteenth century anthropologist Tylor: "The uncivilized man thinks that lots or dice are adjusted in their fall with reference to the meaning he may choose to attach to it, and especially he is apt to suppose spiritual beings standing over the diviner or the gambler, shuffling the lots or turning up the dice to make them give their answers. This view held its place firmly in the middle ages, and later in history we still find games of chance looked on as results of supernatural operation."[25]

Roberts and associates have furnished a more precise argument in favour of the view that games of chance are specifically connected with supernatural categories. They attempted a statistical test, upon nineteen cultures in the Cross-Cultural Survey files which contained adequate information on the presence of such games, of the following hypothesis: "If games of chance are associated with control of the supernatural, they ought to be associated with the belief that the gods are benevolent, with the belief that the gods can be coerced, and not with the belief that the gods are aggressive."[26] The data seemed to support the hypothesis.

That not only games of chance, but also chance-like omens, oracles and divinations have been used in large measure to test out the supernatural, appears likely from much material in Frazer's "Golden Bough". He reports a large number of omens and divinations concerning marriage, death and other important events. What these omens had in common was that, to the people in question (as to us), no natural link of cause and effect existed between the device from which predictions were derived, and the likelihood of the predicted event occurring. Relative to the omens drawn, these tests were arbitrary. The relationship between the test and the predicted event was random on the basis of known empirical laws. Most likely it was precisely this, although not this alone, that gave them their social function, a function which concerns the contact with the supernatural. Either the test served to coerce the gods and obtain reassurance concerning an uncertain future, in accordance with Malinowski's theory of the psychological functions of primitive magic. Or the function was the more modest one of making predictions by drawing omens, and then seeing whether one had been successful in establishing rapport with the supernatural, irrespective of whether the omen was good or bad.

It is possible that we should also include, among the chance devices which serve to provide communications from the gods, phenomena such

as runic inscriptions.[27] Old Norse runes served magical functions, and mastery of the runic art was very different from present-day literacy. Runic inscriptions often have many meanings, depending upon the scheme of interpretation applied. Two letters might be collapsed, one hidden in the other; or the message might be cryptic, in the sense that individual runes could be substituted for others according to pre-established systems of codes. From the point of view of communication between persons, these characteristics of runic inscriptions seem unnecessarily dysfunctional; but maybe not if we look upon the runes as means of contact with the supernatural. What is important here is not primarily that the receiver of the message would be in doubt and risk surprise by hidden meanings. But also the sender of the message, the inscriber, would be somewhat ignorant of what he was doing. His message might have meanings, according to one scheme of interpretation or another, which he had not foreseen. In other words, through the runes spoke not only men, but also an agent independent of human knowledge and volition, the supernatural. It is characteristic that a great many runes contain a term for protection, (alu), which may well refer to protection against unknown dangers inherent in the rune-writer's own text (meinruner), runes that might become magically turned against himself.

In societies, or segments of societies, which have lost faith in the blatantly supernatural, chance devices may still be used to consult with Fate on vital issues, often issues of life and death. Weiss concluded on the basis of interviews with 156 people who had attempted suicide: "Many suicidal attempts have at least in part, the character of a gamble with death, a sort of Russian roulette, the outcome of which depends to some extent, on chance. The attempts are consciously or unconsciously arranged in such a manner that the lethal probability may vary from almost certain survival to almost certain death; and "fate", or at least some force external to the conscious choice of the person is compelled in some perhaps magical way to make the final decision."[28] And, referring to the work of two other psychiatrists: "Stengel and Cook have noted that the outcome of the attempt is almost invariably accepted for the time being and further attempts are rarely made immediately even if there is no lack of opportunity. The outcome of the attempt is accepted like that of a trial by ordeal in mediaeval times".[29]

A case parallel to the one I have made above concerning man's attempts to get "answers" from the supernatural or from Fate, can be made for man's relationship to Law. Ordeals are, at least in part, chance devices, the outcomes of which are interpreted as the judgment of God.

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A decision which is unintelligible and unpredictable in terms of human volition and natural laws, but which is nevertheless definite, must, by some simple principle of elimination, express the judgment of higher authorities.

I should like to close this section with a few remarks on the game of solitaire. Solitaire may be used as a kind of divination, from which omens are drawn about marriage, acquisition of wealth and what not. But it seems especially pertinent in the present context that the playing of solitaire is a means by which even the isolate can communicate. Through manipulation of the cards, in accordance with the rules, the lonely player transmits messages and impinges upon his environment, such that unforeseen, unintended, uncontrolled and surprising "answers" are fed back from the cards. The meanings do not, except in cases of bad cheating, come from the player, but from something or somebody else. Thus, the playing of solitaire becomes a simple and ingenious way of relieving loneliness. It may be related to the same function of relieving loneliness when we find so many superstitious beliefs in magic and omens among lonely dairymaids, lumberjacks, sailors and fishermen.

Innovation and creativity

By innovation I refer to new relations between people. Such new relations often spring out of new relationships between man and nature after a change in climate or a technical invention. Because society is incapable of reproduction and survival on the basis of a set of social bonds established once and for all, creation of new ties are a functional prerequisite of any society, human or animal. And because man's environment is unstable, old social bonds must be capable of transformation, permitting adaptation to new conditions. If social innovations are a functional prerequisite of society, so are normative patterns bestowing legitimacy upon these innovations. Man cannot just act; he must be capable of giving reasons for his actions. And he must be capable of giving reasons *before* he acts.

In most societies of the world other than our modern industrial one empirical knowledge has been very limited, and man's capacity to shape the future according to his own volition was likewise limited. The arsenal of "rational" reasons for doing things is severely limited in primitive cultures, and chance happenings are not accepted. How then, if man cannot act without knowing why, are such societies to achieve a sufficient degree of elasticity in social relations and to avoid a rigidity which ex-

poses it too much to the variability of an environment which lacks respect for normative structures? Somehow, there must be available reasons, arguments, principles of decision for actions that so far lack a good empirical, utilitarian rationale.

Going back to the hunting magic among the Naskapi of Labrador, I would be inclined to interpret the practice as a means of breaking routine, of doing something new, not only in order to surprise the game, but as a way of achieving a wider experience, from which new systematic knowledge may accrue. The technique of divination is a primitive method of trial and error in cultures which do not accept trial and error. The method gives acceptable reasons, drawn from the folkloristic arsenal, for doing things which have the primary function of widening the sphere of experience. It is a primitive experiment, which, if successful, may be included in the commonly accepted stock of rational experience, knowledge of natural laws.

A random device is functional from the point of view of innovation because, in the absence of empirical knowledge, the best bet is the randomly distributed response. Such a mechanism may be operating in a fishing magic from Norway, the use of the "fish-tray". On the bottom of a tray is drawn a simple map of the coastline with the major fishing banks outlined. The tray is filled with water. Small bubbles of air will rise from the bottom to the surface. The location of these bubbles determines where the fish are most likely to be caught.[30] In this case the element of surprise is not likely to be important as it was in the Naskapi example. But the device is essentially similar and of a chance nature. I believe that the major function of the "fish-tray" may be to provide a decision-making device in the face of great empirical uncertainty, and one which permits some leeway from routine operation.[31] The sociologically significant aspect of these kinds of divinations is that they establish between family members or co-workers rights to carry out activities that also concern others, in a non-routine fashion.

If divination in the shape of objective contrived devices, like the fish-tray, may serve the function of breaking routine, so may pre-established codes of interpretation of natural events. A possible case in point is the codes for dream interpretation. In instances where dreams, without forethought, are confronted with pre-established codes,[32] "dream books" or the like, the codes may resemble the modern lists of random number. Theories of dreams may serve the innovative function in society by multiplying the number of reasons for doing things beyond that which is empirically warranted or morally permitted without dream justification.

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There exist "lists of random numbers" for the interpretation of other natural events. Astromedicine is one example, based as it is upon the (empirically unfounded) assumption of definite relationships between celestial phenomena and the functioning of the human body. From astromedical laws were derived a number of principles for the intervention of a doctor in the state of health and sickness of his patients, medication, surgery and other kinds of treatment. These interventions were, in part, completely random, leading to similar prescriptions for entirely different diseases, and different treatments of the same disease, because of the (as we know) largely random relationship between human and celestial phenomena. But the innovative function may have been served by the astrological and other magical beliefs of early medicine. It does not seem unlikely that Paracelsus' achievements were, in part, due to the vast store of astrological and magical belief upon which he drew.[33] His unscientific beliefs gave him reasons for a ceaseless series of breakneck experiments. But out of these experiments grew empirical knowledge, modern medicine. A similar case may be made for the innovative function of the random hodge-podge of beliefs upon which the art of the goldmakers rested, and out of which chemistry developed. The sheer number of culturally accepted beliefs, formulas, statements, assumptions, appears essential in relation to creativity and innovation. If this view is sound, it opens up a new perspective upon the social function of non-rational thought in general. It may possibly give us a key to the understanding of such phenomena as water-witching, still widely practised in the U.S.A. and other industrialized societies. Vogt and Hyman interpret it as an anxiety-reducing ritual practice employed in the face of great empirical uncertainty about the location of underground water.[34] In terms of the present analysis it seems that water-witching serves as a legitimizer for random behaviour, where randomness is rational, but where "trial and error" is unacceptable as an explicit value basis for action partly because of the high costs of drilling new wells.

The innovating functions of divination, games of chance and magical belief of the random number type are in modern industrial societies often served by other mechanisms. The *chance ideology* has taken the place of the cumbersome objective devices; the conscious notion that experiment, trial and error, originality are values in themselves within the institution of sience.

The most famous and systematic modern ideologies of science tend to avoid the innovative aspect of scientific work. Logical positivism depicts the scientist as a producer of statements, verifiable, communi-

cable and true. The niceties of this ideology have a purely retroactive function. They provide norms for evaluating scientific statements that have been made already. Our problem is to locate the norms and mechanisms by which it is possible to legitimize in advance the production of previously unverified or not accepted statements. It is commonly recognized that logic or "the logic of science" gives little or no clue as to how ideas are to be produced, but it is restricted to laying down rules about how they are to be handled once they have been produced. There is in the institution of science a fundamental ambivalence, a tension between truth and novelty. Ideas are, sociologically speaking, either true, that is, communicated and accepted, or novel. They can be both only in a brief moment when a new discovery is being proven.

The value of originality or novelty is, in the nature of things, much less systematized than the value of truth; although truth is a value which science shares with many other institutions, while the emphasis upon originality is unique to science, a new value. As Whitehead put it, man made his greatest discovery when he invented the method of invention.[35] The institution of science was born when the value of novelty, invention, creativity, discovery and originality had become sufficiently embedded in a culture to motivate large numbers of well-equipped people to dedicate their lives to the production of new ideas. The scientist became then, not so much a man in search of truth, as a man who was permitted, forgiven, even encouraged and praised for making many false statements, as long as he did not abandon his basic attachment to the value of truth.

There is no stronger testimony to the institutionalization of the value of originality and novelty in science than the recurrent priority fights that have ravaged scientific communities for centuries. Merton has recently given a historical exposé of priority strifes from the sociologist's point of view, and emphasized their functional significance in enforcing the value of originality.[36] Another recent writer on the social system of science stresses the values by which "pure scientists" are ranked and rewarded: "The core of the value system of pure science consists of two related beliefs: first, that new knowledge should be evaluated according to its significance for existing theory, and second, that scientists should be evaluated according to their contributions of new knowledge. Highest honours go to those whose work involves radical reformulations or extensions of theory or conceptualization. Next come those who do the pioneer experimental work required by a theoretical reformulation. Next come those who carry out the work logically required to round out the

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conceptual structure. Next come those who carry out redundant experimental work of a confirmatory nature, or concern themselves largely with relevant data accumulation. Last are the doers of sloppy and dull work."[37] According to this scheme of ranking, it is the originality and not the degree of adherence to truth (which may be equal in the four first groups) that distinguishes scientists from each other in terms of merit.

Whether or not scientific progress has been characterized by chance innovation or not, it is a widespread belief within the scientific community that discoveries and inventions depend in large part upon luck. As a supplement to the common notion that science demands systematic and unusually hard work, the "happy accident" has also become an article of faith. Many great creative scientists and artists have put much emphasis upon the chance event as initiator of a discovery or an invention.[38]

Ernst Mach said in his paper "On the part played by accident in invention and discovery": "It is well to ask, are we justified in placing a low estimate on the achievement of an inventor because accident has assisted him in his work? Huygens, whose discoveries and inventions are justly sufficient to entitle him to an opinion in such matters, lays great emphasis on this factor. He asserts that a man capable of inventing the telescope without the concurrence of accident must have been gifted with superhuman genius."[39] . . . "The disclosure of new provinces of facts before unknown can only be brought about by accidental circumstances, under which are *remarked* facts that commonly go unnoticed."[40] . . . "I do not know whether Swift's academy of schemers in Lagado, in which great discoveries and inventions were made by a sort of verbal game of dice, was intended as a satire on Francis Bacon's method of making discoveries by means of huge synoptic tables constructed by scribes. It certainly would not have been ill-placed."[41]

A more contemporary writer on the same topic emphasizes, with some misgivings, how widespread the belief in the happy accident is, even outside the scientific community: "The radio, popular literature, the loose parley of the streets, all exalt such moments in the course of human progress as Goodyear's "lucky" and chance discovery. Not so prevalent, however, is the emphasis on all the substantial progress, all the hours of dull, minute and precise scientific prying that eventually crystallized in that sparkling moment of the spilled mixture."[42] Nevertheless, the writer ends up by giving new credence to the belief in the chance basis of significant scientific discoveries.

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What concerns us here is not whether the above statements are true or not; but whether these beliefs in the happy accidents are representative and express something significant in the ethos of the scientific community. I shall assume that such is the case.[43] On the basis of our previous reflections it seems possible to guess what functions are served by this belief. The scientist faces the problem, if he wants success, of investing a long education and arduous work in the pursuit of novelty, of original ideas, the truth and social acceptance of which he may only make uncertain guesses about. The social situation of the gifted and ambitious scientist has been that the stakes involved in the proper performance of his role are very high, and the likelihood of reaping the fruits of the work is unpredictable. In such a situation a doctrine of luck may serve much the same function as in highly competitive social structures of other kinds.[44] It reduces the probability of investing ability and hard work in pursuits, for the possibly unsuccessful outcome of which one has to shoulder the responsibility. In some measure the homage paid to the "happy accident" bestows freedom of responsibility for failure upon the daring adherent of the novelty value in science. It prevents failure from being interpreted as a reflection of inability, and it may protect the successful from unbearable envy. In short, the doctrine of chance in science seems to be a necessary ideological link between the partly contrasting values of truth and novelty.

Representativity

If we look upon social bonds as patterned relations between Egos and Alters exchanging performances, it will often happen that only a sample of possible Egos can perform, or that the Egos can only respond relative to a sample of all possible Alters. Thus, some select few will have to govern on behalf of a larger group; and the police can only detect and punish a few of all those who break the law. How are Egos and Alters selected in situations presenting such a sampling problem?

Let us first look at the problem from the side of the actors, the Egos. It presents itself, first of all, as the question of how to decide who shall make decisions *on behalf of* a group or category of persons. It is the problem of government and power.

The classical example of the selection of rulers through a chance device is the use of the lot in the Athenian democracy. The population of the city districts, the demes (there were about 100 of them) proposed lists of candidates from which Athenian jurymen and Athenian

councillors were selected by lot. The demes elected candidates for membership of the Council (of which there were 500 in all) on a proportionate basis according to the number of inhabitants. The eligibility of the candidates was dependent upon passing a test of their qualifications. When the qualifications had been determined the final selection for a Council seat was made by lot.[45]

The system of selection to the Athenian council consisted in a blend of three principles: the choice by a population, determination of innate qualities and, finally, random selection. As for the functions of the last element, Barker makes this suggestion: "In fact, the qualification of election by the additional use of the lot was perhaps largely due to the desire to avoid election intrigues."[46] The use of the lot was criticized both by Socrates and Plato because it gave an equal chance to those unqualified to govern. Their opposition shows, by the way, that they did not trust the lot as an instrument of the deity.

A modern example of the use of chance devices to select political leaders is to be found in the small European state of San Marino, known for its famous Casino. San Marino is governed by two *capitani regenti*, traditionally elected by the 60 Man's Council of the republic, the *arengo*. The arengo designates twelve nominees that again designate their candidates. They vote for candidates by means of white and black balls, and the six who receive most white balls are further divided into three pairs of candidates. These three pairs are then led in a procession to the cathedral where the archbishop receives them. Then an innocent child of San Marino draws from an urn one of three scraps of paper furnished with two names — and the republic has been provided with two new governors.

In modern times, however, the judiciary is the branch of government where chance devices have been most frequently used as a mechanism of recruitment to positions of authority. As the jury system (and a system of lay judges) is practised in Norway, however, selection of jurors is based upon mixed principles. A general supply of prospective jurors is deliberately chosen by municipal authorities. In other words, the general recruitment to the roles of jurors is not a chance happening in any strict sense, although much "arbitrariness" may be involved. What is a matter of chance, although not without modifications, is the decision as to who, among a group of candidates, should pass judgment on the question of guilt in any particular case. In other words, what has been left to the mercy of chance is the decision as to who, given a possible deviant, should be the sanctioning agent vis à vis *him*.

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The custom of using chance devices to select incumbents of positions of public authority is not restricted to cultures which have been influenced by the democratic tradition originating in Athens. Frazer gives several accounts of how rulers are pointed out by divination, including, in the case of the election of the Dalai Lama of Tibet, the drawing of lots from a golden jar.[47] Another custom that provides for a random principle in the selection of rulers has to do with the institution of the Saturnalia. Frazer believed that the Latin kings were sons of unknown fathers, their mothers having become pregnant in the course of a Saturnalia, during which time they were promiscuous.

Through divination, the drawing of lots or even the Saturnalia, the gods were permitted to point out the rulers, thereby giving the government a sacral touch. That this may be functional from the point of view of establishing authority seems fairly clear.[48] Selection for office by chance may also reduce political strife and competition for power. Emulation and display of strength of gifts, or the rallying of supporters is unnecessary and actually useless. In the Athenian democracy, with its fear of hubris, this line of reasoning seems even to have been quite explicit. By making the terms of office brief and the incumbency dependent upon random mechanisms, a counter force is built up against political ambition. Scheming for power is rendered futile. If Ranulf is anywhere near right in his analysis of the climate of opinion under the democracy of Athens, with great emphasis upon equality, much envy and interpersonal aggression,[49] it would seem to give a good explanation of the election to political office by lots.

Election to a jury by lot may be viewed as a just way of distributing inevitable burdens. But it is also perceived as one means by which the independence and objectivity of the courts are secured. The juror, like the lay judge, is the representative of the people in an activity which is largely professional and therefore hard to control. Since he is untrained, he performs his role under some suspicion of emotionalism and partiality. The election by lot secures, however, a minimum of independence. Nobody can designate him with a view to his subjective disposition vis à vis one or the other of the contending parties. That decision is left to chance. Although this gives no guarantee that the juror will be an unbiased person, it does prevent the occurrence of systematic biases in any particular case, both in fact and in appearance.

Let us now turn to the sampling problem in power and government as it presents itself from the other side. If social control is to be exerted, rules enforced within a collectivity, how are the specific targets of

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sanctions to be selected? There are two main reasons why direct social control of everybody standing under the force of a social norm is difficult or impossible in a great many situations. Reasons of "economy" often make it impossible for every case of deviance from a norm to be punished, even if they are known to the agencies of social control. Secondly, many deviant acts remain hidden to the sanctioning agents, thus preventing enforcement. The magnitude of these problems of social control, created by secrecy of deviance and need for economy with sanctions, is very forcefully brought to our attention by the now existing statistical data on so called "hidden criminality".[30] According to the best available information most criminal acts remain unknown to the agents of law enforcement. And if they were to become known and punished, it would imply an expansion of the watchdog institutions of society beyond the limits of what the social system could endure.

If, from a universe of deviant and partly hidden acts, a relatively small sample has to be selected for exposure to sanctions, one can give two theoretical arguments in favor of random social control. If we assume perfect ignorance of the distribution of deviance within a group of actors, which is only a marginal case, any systematic principle for the selection of the accused is exposed to the risk of going systematically wrong, hitting deviance less frequently than it hits conformity. The consequences would be that groups of deviants, if we assume less than perfect ignorance of sanction policy on their part, would learn that they had little to fear. Social control by random terror, like shooting every 10th person in a town, may rest upon considerations of this kind.

Ordeals are sometimes reported to have been used to detect the guilty person when ignorance was great and no specific person was suspected. The group of possible perpetrators were collected and exposed to the test. The ordeal has the advantage that the guilty person is at least exposed to a risk of sanction and the ordeal appears to demonstrate that what is hidden to man, is nevertheless revealed to God. It is paradoxical that the vehicle by which God's "limitless vision" should be expressed is a device of "blind chance".

Suppose that a thousand criminal acts are known, or easily knowable, to the law-enforcing agents. If there is only time and personnel available for handling a hundred cases, which ones are to be selected? The answer in terms of justice would be to rank the sets according to their seriousness, and sanction the hundred most serious offences. And, actually a higher number of murderers and robbers are sanctioned than of petty thieves and pilferers. But a rigid adherence to "justice" in this sense

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means that the police would have their hands full with the most serious offences, and the less serious offences would go systematically free of sanctions. This situation is intolerable, since large numbers of completely unsanctioned small offences may bring total disorder to vital institutions in an urban industrial society.

What actually develops is a compromise between the principle of justice and random distribution of threats. The spread of sanctions to cover the many petty offences as well is not, in modern society, achieved by chance devices in the objective sense. It is achieved by decentralization and specialization of control agencies, that is by ignorance and barriers of communication. The criminal police are so dedicated to the idea of worst things first, that if control of socially important, but undramatic offences, like price violations, etc., are left under their jurisdiction, these norms will be enforced with insufferable laxity. So, a vast number of independent or semi-independent organs of legal control have developed, each specializing on certain types of deviance. Justice has become specialized, because rigid justice is something which society cannot afford if it is to survive.

In primitive societies the problem of sanction economy is solved in a more elegant fashion. They draw upon the natural supply of suffering by ingeniously transforming them into morally meaningful sanctions. They achieve this by their theories of disaster, above all, their explanations of illness and death. Suffering, illness, or death may be viewed as the consequence of deviance on part of the sufferer, in which case fear of falling ill may be a deterrent against moral transgressions. Or, illness and death may be viewed as a consequence of black magic or sorcery on the part of somebody else. This theory is a two-edged sword. It makes people afraid of incurring enemies because enemies may give them diseases through sorcery, and also because they are afraid lest their enemies fall sick and accuse *them* of sorcery. This implies that the serious offender, the petty offender and the relatively blameless citizen stand an equal chance of being sanctioned. Such a system of sanctions can, by virtue of its randomness, not teach anybody the difference between right and wrong. But given that the cognitive discrimination between right and wrong is learned already, theories of illness, just like police terror, create a general feeling of uncertainty, fear of doing anything offensive, since no-one knows exactly what it takes to release these supernatural sanctions. And the randomness of the system has the consequence that it serves no purpose to hide offences — secrecy is no protection against sanctions.

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But if sanctions hit individuals in a random fashion, will it not be empirically observed that there is no relationship between merit and reward, sin and punishment? In the first place such observations are hard to make. Furthermore, people are in general sinful in the sense that they are aware of something for which they might deserve punishment. Therefore, any case of sickness, or accusations of sorcery may be viewed as a confirmation of the theory by the sick person.

Equality and justice

There is one, and only one area of life into which all men are born equal, and also remain equal throughout their lives, independent of physical, pecuniary, intellectual, or moral achievements: the pure game of chance. Small wonder then that so many people representing so many different cultural backgrounds have sought refuge from the inequalities and injustices of real life in this model of an egalitarian society. Interpreted as divination, as we have seen a fairly universal pattern, it creates an egalitarian distribution of God's grace. The egalitarian distribution of "answers" from chance devices may, on the other hand, be one of the reasons why a religious interpretation of games of chance and other chance devices has become so widely accepted.

What are "equality" and "justice" in our present sociological context? One possible answer, a Platonic answer, is that equality means that equal contributions, merit or failure, are met with equal sanctions, rewards or penalties. To further this kind of equality or justice, chance devices are by definition unsuited. The establishment of this kind of justice suffers, however, from disequilibriating tendencies. It is a constant problem, as we see in all negotiations concerning wages, to reach and retain consensus on the criteria of performance on the basis of which the sanctions should be adjusted. In other words, with respect to what criteria are individuals to be grouped as equal or unequal?

There exists another concept of equality, however, which makes it possible to avoid this dilemma by assuming that all men are equal, independent of special characteristics. It brings to the fore another method of establishing and observing equality and justice. "Independence" is the key word when absolute concepts of justice are involved. One way of achieving independence between "in-put" in a decision-making agency, and the "out-put", the decision, is to insert a randomizer, a chance device. What is then achieved is not that each contribution or merit

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receives its just reward, but that it can be established that rewards and penalties are at least unrelated to those criteria that are deemed as irrelevant biases.

Under what conditions is each one of the two types of equality adapted to further harmony within a social system? In order to practice justice in the sense of "to each according to worth and merit", two conditions must be fulfilled. There must exist a reasonable consensus concerning the criteria of merit; and it must be possible in the individual case to observe whether the criteria agreed upon are fulfilled or not. In situations where these two conditions are not present, we might expect the occurrence of chance devices, or of chance theories.

The classical example is trial by ordeal. It took place in a context where there was general consensus about what sanction should be adjusted to what specific types of performance, the crime of the person with regard to whom a decision had to be reached. But there was ignorance concerning the presence or absence in the individual case of the relevant criterion, the guilt. We have come to solve such problems by increased fact-finding skill on the part of a professional agency, the police, and by the rule *in dubio pro reo*. Both of these factors are products of the sophistication of urbanization. If they are non-existent, by what method can the court establish a just relationship between deed and sanction? It seems that a chance device is the only available means, because it does secure, and give the appearance of, independence between sanction and irrelevant characteristics of the defendant and of the judges. On the manifest level the ordeal serves to discover the facts of guilt or innocence by divine intervention, thus permitting an application of the Platonic concept of justice as a basis for meting out punishments. On the latent level the ordeal demonstrates the independence between the decision and possible prejudices of the judge or particularistic relationships between defendant and judge. It may be claimed that as far as *apparent* independence goes, no chance device is needed, only an appeal to sources apparently outside the control of the judge. Both oaths, battle and falsified ordeals may serve this function. The divine chance device is only one among several mechanisms that may serve to uphold the court's claim to be a dispenser of justice, through the apparent independence between outcome and existing biases. It is however, superior to the others in the sense that in the long run it can be statistically observed that there is no correlation between these factors.

When we conclude that decision by chance mechanisms is functional when the problem of individual guilt or merit is uncertain, it must be

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recognized that it rests upon one further assumption, which may appear obvious. A chance device is only useful if we assume that the decision sought must effect *uneven* distribution of sanctions. And this carries us back to the control aspect of trials. Trials serve not only to achieve justice or equality, but also to threaten, by demonstrating the different consequences of different courses of action on the part of the citizens. We see this most clearly in the old cases where a limited number of people had been pointed out as suspects, and the choice of the guilty one was made by submitting them all to ordeals. It is close to axiomatic under such a condition that they cannot all be treated equally in the sense that they will all get the *same* treatment. To uphold the law, there must be an uneven distribution of sanctions.

This is, however, not a necessary constraint in all situations where an agency is faced with the problem of distributing sanctions under conditions of imperfect or non-existent knowledge of relative guilt or merit within a group. There are only two types of situations where mechanical equality of sanctions is ruled out as a way of coping with ignorance concerning contributions of the actors. There is the situation mentioned above, where differential deprivation or reward is functional as a means of social control or socialization. The other type arises out of scarcity and indivisibility of the advantages and disadvantages which are to be distributed. Exemption from military duty on the basis of lot-drawing is well known from several countries before the last war. The government took recourse to a chance device, not so much because there was ignorance with regard to the presence or absence of merit in individual cases, but rather because it appeared highly doubtful what criteria determine whether a young man "deserves" to be enrolled or not. Had it been possible, and practical, to divide the "disadvantage" indefinitely, no sampling would have been necessary, and a solution in terms of absolute mechanical equality would probably have been sought.

Nowhere is the close relationship between chance and justice brought more forcefully to our attention than in children's games and some of their ways of coping with the problem of distributing scarce values. From an early age, children learn that many, and to them important decisions, are best made by random devices, like saying "eenie, meanie, miney, mo," when selecting players for a specific role in a game, by drawing straws when dividing pieces of chocolate, etc. Parallel to the indoctrination of the achievement pattern as a basis for later adult conceptions of justice, we find a development towards an adult sense of justice through a learning to observe the rules of pure chance.

NOTES

[1] *The works of William Paley,* London, 1801, pp. 255–56.

[2] *Op. cit.* p. 257.

[3] Omar Khayam Moore, "Divination ... A new perspective." *American Anthropologist.* Vol. 59, 1957, p. 72.

[4] Henry E. Sigerist, *The history of medicine.* Vol. I. New York 1951, p. 127.

[5] Leo W. Simmons, *The role of the aged in primitive society.* New Haven, 1945. pp. 217 ff.

[6] Cf. Bronislaw Malinowski, *Magic, science and religion.* A Doubleday Anchor Book. New York, 1955.

[7] Wilbert E. Moore and Melvin M. Tumin. "Some Social Functions of Ignorance." *American Sociological Review.* Vol. XIV, 1949, p. 788.

[8] *Op. cit.* p. 788.

[9] *Op. cit.* p. 790.

[10] *Op. cit.* p. 790.

[11] *Op. cit.* p. 791.

[12] Robert K. Merton. "Social structure and anomie". *Social theory and social structure.* Glencoe, Ill. 1949, p. 139. Empirical studies have indicated more readiness to explain success by *luck* among lower class respondents than among respondents with higher rank. Cf. Richard Centers, "Attitude and belief in relation to occupational stratification." *Journal of Social Psychology,* 1938, 27, pp. 159–85.

[13] Vincenzo Cioffari, *Conception of fortune and fate in the works of Dante.* Harvard Univ. Press. Cambridge, Mass. 1940; *and* by the same author, *Fortune and fate from Democritus to St. Thomas Aquinas.* New York, Privately printed, 1935.

[14] John M. Roberts, Robert R. Bush, and Malcolm J. Arth, *Dimensions of mastery in games.* Center for Advanced Study in the Behavioral Sciences. (Mimeo.) 1957.

[15] Cf. Johann Huizinga, *Homo Ludens.* London: Kegan Paul, 1943. Cf. also John Cohen and Mark Hansel, *Risk and Gambling.* London: Longmans, Green and Co., 1956.

[16] In this respect our approach differs from that of Huizinga and also from that of Roger Caillois, "Unity of play: diversity of games". *Diogenes.* Number 19, 1957, pp. 92–121.

[17] Paul Vinogradoff, in Art. "Ordeal". Hastings, *Encyclopaedia of religion and ethics.* Vol. 9. Edinburgh. T. T. Clark, 1917, p. 519.

[18] Hermann Nottarp, *Gottesurteile. Eine Phase im Rechtsleben der Völker.* Bamberg 1949, p. 12 ff.

[19] Cf. Nottarp, *op. cit.* Vinogradoff, *op. cit.,* and H. af Trolle, *Om ordalierna hos de germanska folken.* Stockholm, 1915.

[20] Charlotte Leitmaier, *Die Kirche und die Gottesurteile.* Wien, 1953, pp. 41 ff.

[21] A. F. Stenler, *Die Indischen Gottesurteile,* 1907. In Bibl. Soc. Orien. Germ.

[22] Joshua VII, 13 ff., Samuel XIV, 24 ff., Proverbs XVI, 33 and XVIII, 18; and Joshua I, 7.

[23] Cf. E. Adamson Hoebel, *The law of primitive man.* Harvard Univ. Press. Cambridge, Mass. 1944, pp. 251 and 262. The use of oracles in witchcraft cases should be included here, cf. E. E. Evans Pritchard, *Witchcraft, oracles and magic among the Azande.* Oxford, 1937.

[24] Cf. also Reo Fortune, "Divination". *Encyclopaedia of the social sciences.* Vol. 5, New York 1931, pp. 174–6; and for a very detailed description of techniques of divination, S. G. Nadel, *Nupe religion,* London, 1954, pp. 38–67.

25 Edward B. Tylor, *Primitive culture*. Vol. 1. London. 1873 (2. ed.), p. 79.

26 Roberts et. al., *Op. cit.*, p. 8.

27 I owe this idea to Tom Broch.

28 James M. A. Weiss, "The gamble with death in attempted suicide". *Psychiatry*, vol. 20, 1957, p. 21.

29 *Op. cit.* p. 24.

30 Joh. Th. Storaker, *Tiden i den norske folketro*. Kristiania, Norsk folkeminnelag 1921, pp. 175–7.

31 Chance devices may be especially needed in situations of great uncertainty, since the alternative predisposition in risky situations is stereotyped, precedent-ridden behavior.

32 Such a code is presented in Thomas A. Sebeok and Frances J. Ingemann, *Studies in Cheremis: The supernatural*. Viking Fund Publications in Anthropology. No. 22. New York, 1956, pp. 269 ff.

33 Cf. Henry M. Pachter, *Magic into science. The story of Paracelsus*. New York, 1951.

34 Cf. Ray Hyman and Elizabeth G. Cohen, "Water-witching in the United States". *American Sociological Review*. Vol. 22, 1957, pp. 719–24.

35 Bernard Barber, *Science and the social order*. Glencoe, Illinois, 1952, p. 192.

36 Robert K. Merton, "Priorities in scientific discovery: A chapter in the sociology of science." *American Sociological Review*. Vol. 22, 1957, pp. 335–59.

37 Herbert A. Shepard, "Basic research and the social system of pure science." *Philosophy of Science*. Vol. 23, No. 1, Jan. 1956, p. 50.

38 Brewster Ghiselin (ed.) *The creative process*. A Mentor Book. New York, 1955.

39 *The Monist*. Vol. VI, 1896, p. 163.

40 *Op. cit.* p. 169.

41 *Op. cit.* pp. 174–5.

42 Franklin McLean, "The happy accident." *Scientific Monthly*. Vol. 53, 1941, p. 61.

43 For confirmatory evidence, see: Barber, *Op. cit.* pp. 191 ff; P. W. Bridgman, "Impertinent reflections on history of science." *Philosophy of Science*. Vol. XVII, 1950, pp. 63–73; Joseph Rossman, *The psychology of the inventor*. Washington D. C. 1931, pp. 117 ff; Arthur Koestler, *The Sleepwalkers*. London: Hutchinson & Co. 1959.

44 See Merton, "Social structure and anomie", p. 139.

45 Ernest Barker, *Greek political theory. Plato and his predecessors*. London, 1918, pp. 34–35.

46 *Op. cit.* p. 35.

47 *The magic art and the evolution of kings*, pp. 411–12.

48 This may have been an important aspect of the Old Norse tradition of having pretenders to the throne, or their mothers, in cases of doubt, submit themselves to ordeals, especially the hot iron tests.

49 Svend Ranulf, *The jealousy of the gods and criminal law at Athens*. I–II. London & Copenhagen, 1933.

50 Cf. Vilhelm Aubert: *Om straffens sosiale funksjon*. Oslo 1954.

THE GENETIC FALLACY AND NATURALISTIC ETHICS

by

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Frequent references to the genetic fallacy are encountered in philosophic literature. Some observers think that it is often committed; it has been described as "a fallacy that has time and again blocked the path to a thorough understanding of logical and philosophical problems".[1] Fairly often naturalists, pragmatists, and materialists are thought to be the chief offenders. It is not unheard of for a classroom lecturer to describe dialectical materialism, say, as "a gigantic example of the genetic fallacy." Especially are defenders of naturalistic ethics said to commit the fallacy.

Many versions of what "the" genetic fallacy is can be found. For example, it has been said to consist in confusing the causal antecedents with the developed character of an ethical ideal or moral emotion;[2] in confusing the social genesis of thought with its truth or falsity; in depreciating the product of an evolutionary process because of its lowly origin;[4] in holding that genetic factors can of themselves validate a belief;[5] or in confusing the analysis of meanings and the causal explanation of facts.[6] It may be noted in passing that in discussions of the genetic fallacy concrete examples are often not given, and specific philosophers are not named.

In respect to ethics, four versions of the genetic fallacy seem most important: 1) that it consists in failing to distinguish between the origins and the developed nature of a moral rule or a moral emotion, 2) that it consists in confusing an analysis of meaning with a causal explanation, 3) that it consists in making the *truth* or *falsity* of a statement depend upon genetic factors, and 4) that it consists in making the *validity* or *invalidity* of a statement depend upon genetic factors. (Often in discussions of the fallacy, no distinction is made between 'truth' and 'vali-

dity.') The remainder of this paper will be given over to a consideration of whether a naturalist in ethics, just because he is a naturalist, will be inclined to commit the fallacy; and to a discussion of the relevance of genetic considerations for ethics.

By a 'naturalistic ethics' in this paper is meant a theory which holds that ethical statements are descriptive, cognitively meaningful, and hence capable of being true or false; and which holds that ethical terms designate natural characteristics, rather than unique, nonnatural, and unanalyzable characteristics. This distinguishes naturalists from the emotivists on the one hand, and from intuitionists on the other.

Two examples will be given to illustrate what might be meant by version 1) above of the genetic fallacy. G. Bergmann says that, assuming psychoanalytic theory to be true, "moral disapproval depends causally, according to such a theory, on what other occasions in our early childhood we experienced or anticipated pleasure or pain."[7] He goes on to say that what we now experience when we disapprove of something is not necessarily either pleasure or pain, their anticipation, or their memory. A. C. Ewing makes some similar remarks. Naturalists, he says, hold that ethical ideas and beliefs originate psychologically from nonethical factors such as fear. Even if this is true, Ewing says, it "would not prove that they now contained nothing beyond these."[8]

It may be doubted whether very many philosophers would actually confuse the developed character of a moral emotion or a rule with its origin. The mere fact that someone allegedly accounts for the present nature of an emotion or rule through an explanation of its origin and development is an indication that he neither identifies the two nor fails to distinguish between them. The naturalist is not obliged to confuse the ethical and the nonethical in the way Ewing suggests. Suppose that a naturalist holds that the key to an understanding of conscience is to be found in the parental commands to the child. He will hardly identify what the subject introspectively experiences as a command of conscience with the parental injunctions. In fact, if the two were held to be identical, it would seem strange to argue that one had developed from the other. Presumably, the naturalist is arguing for some type of continuity between the origin of a moral rule or a moral emotion, and the way it now functions.

In regard to version 2) of the genetic fallacy, it is no doubt possible to confound an analysis of meaning with a causal explanation of facts. But a naturalist need not fall into such a confusion. What he is likely to assert is that genetic factors have some bearing on the plausibility of a

given theory or view. By 'right,' some individual may mean 'what is commanded by God.' One could very well argue that a causal explanation of how this meaning came to be accepted was pertinent to ethical theory, without in the least confusing meaning and causal explanation. Or consider the belief that wives ought to obey their husbands, but not vice versa. It is likely that both the origin and the development of this belief can be explained by a cultural anthropologist. Assume for a moment that socio-economic circumstances are sufficient to explain the genesis of the belief. One can hold that this may be relevant, in some sense, to the correctness of the belief, without confusing the belief's meaning with its origin and development. Perhaps what is also involved here is the difference between those who want to make the analysis of the meaning of terms central to the philosophic enterprise, and those who prefer a broader view.

The third interpretation of the genetic fallacy calls for a longer discussion. A naturalist may hold to some form of the correspondence theory of truth. If so, the truth or falsity of a statement is an objective matter for him; genetic factors need not enter into consideration. Examples from two sources friendly to materialism will emphasize this point. . A. Edel has argued that all ideas (both true *and* false) have socio-historical causes; that most ideas have socio-historical functions; and that many ideas have a socio-historical content. He strongly insists, however, that "the truth or falsity of ideas is a thoroughly objective question not to be confused with the preceding propositions."[9] H. Freistadt has said: "Politics and history have no bearing on the truth of a scientific or philosophical proposition, but may have a bearing on why or how that proposition came to be advanced," and on the ideological and historical role it plays.[10] Naturalists, then, may feel that it is philosophically relevant to investigate the causes for people believing true or false statements, and the social role of those beliefs, without making truth or falsity depend on causal factors.

More pragmatically oriented naturalists may hold a somewhat different view of truth. J. H. Randall argues that in the actual procedure of scientists, beliefs are "warranted," "verified," or proved "true" when "they give a satisfactory solution to the specific problem to which they have been proposed as an answer." In the scientific context, beliefs perform some function. He continues: "any functional test of the validity and value of a belief does involve knowledge of the genesis of that belief, of its history." The genesis of a belief, according to Randall, determines the specific problem which the belief is intended to satisfy,

the context and conditions in which the belief functions, and the nature of the functions it performs. Therefore, genetic considerations do not of themselves either "validate or invalidate a belief," but they are involved.[11] Either on the correspondence theory of truth, or on an instrumentalist theory, then, truth and falsity are not determined by genetic factors.

There are some cases in which becoming aware of the genesis of a moral rule may be a reason for no longer believing that rule to be true. Suppose that moral rule X is held to be correct because a) God has ordained it, or b) it has been accepted universally and for all time, or c) it is a part of the structure of the universe. If in any of these three cases it can be shown through a genetic inquiry that the rule had a temporal, human origin, and that it arose in a specific socio-historical context, the correctness of the rule is seriously questioned. Such cases may not be of great interest to technical philosophers, but they are important for popular morality. A genetic inquiry, then, while not proving that a certain rule is false, may effectively refute arguments which are sometimes given in support of a rule. In this sense, genesis and truth have some relation.

Earlier it was said that specific examples of the commission of the genetic fallacy are often not given in discussions of the fallacy. It is sometimes suggested that the "confusion" of genesis and truth can be found in the writings of naturalists who attempt to relate ethical theories to their social context. Two examples of such attempts will be given. The first is J. Huxley's view on the relation of slavery, morality, and society.[12] This is a likely place to find an example of the genetic fallacy, especially since his book is neither precisely nor carefully written by current philosophic standards. Huxley argues that unless there are appropriate social and material conditions, an ethical principle will remain unformulated, or be so formulated that it will not bear upon the problem at issue. He mentions that neither Jesus nor Paul denounced slavery as such; slavery was taken for granted and even supported by early Christian moralists. Even though the universalist and Christian principle of respect for the individual was opposed to slavery, slavery was a necessary element for production. Therefore, this principle could not be formulated as demanding the abolition of slavery. By the nineteenth century, the progress of technology made it possible to eliminate chattel slavery, and free workers in industry proved to be more efficient than slaves. Under these social conditions, the principle of human equality could be applied to the abolition of slavery.

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The correctness of Huxley's view is not at issue here; the question is whether he makes the truth of a moral principle dependent upon causal factors. It seems clear that he does not. He does not say that slavery was morally correct under one set of conditions, and immoral under another set. What he does say is that ethical principles may not be applied if material conditions are inappropriate. Huxley also seems to believe that certain moral rules are correct even if they cannot be applied at some specific place and time. He says our problem is to construct an ethical system adapted to our age, but also "compatible with the general ethical standards we have been able to distil out of evolutionary fact."[13] Although there is an intimate relation between social evolution and moral ideas, according to Huxley, the truth or falsity of these ideas is independent of their evolution.

Another, more philosophically sophisticated, example of a genetic approach is illustrated by A. Edel's comments on the recent history of American naturalistic ethics. He argues that there have been three distinguishable periods in American naturalistic ethics in this century, and that these trends "reflected clearly the major movement of twentieth century American historical development."[14] The first period, typified by R. B. Perry and J. Dewey, was an expansive one which corresponded to the expansive character of American life before the first World War. The second period was one which put an emphasis on the arbitrary and the emotive; it corresponded to the disillusionment resulting from depression, unemployment, class conflict, the rise of fascism, and war. Reason seemed incompetent to many and democracy inefficient. After three years of depression in America and in the year Hitler came to power, Charner M. Perry and Donald C. Williams published articles attempting to base morality on arbitrary choice. (Perry: "The Arbitrary as Basis for Rational Morality," *International Journal of Ethics,* XLIII, 1932—33. Williams: "Ethics as Pure Postulate," *Philosophical Review,* XLII, 1933.[15]) This relativistic outlook was modified by many during World War II. Nazi barbarity, the devastation of war, and other social factors helped to show the common need of all people for peace and economic productivity. In the third phase of naturalistic ethics, a renewed search for objectivity, and an effort to relate values to the social context of life, occurred.

It should be emphasized that Edel does not say that any of the types of naturalistic theories he discusses are either correct or incorrect because of their causal antecedents. He obviously thinks the emotive theory is incorrect, but he does not so regard it because it reflects certain conditions

of society. He also avoids any suggestion that a theory may be true under one set of social conditions and false under another. He holds, in fact, an opposing thesis: that the arbitrary relativist view was always incorrect. Edel's interpretation of the relation of naturalistic theories of ethics to social conditions may be errroneous, but the error, if any, is not that of the genetic fallacy.

The two examples just cited of naturalistic attempts to explore topics in ethics using a genetic method illustrate that some naturalists, at least, are able to avoid falling into genetic fallacy. It may be possible that some of the furor over the genetic fallacy is due to a hostility toward the use of the genetic method, or a feeling that this method is not an appropriate one for philosophy.

Attention may now be turned to the fourth version of the genetic fallacy: that it consists in making validity depend upon a genetic inquiry. In the strict sense of 'validity,' it is doubtful that validity is often confused with genesis. Yet the two may be related in some cases. W. D. Ross, who is certainly antinaturalistic, comments: "It is often said . . . that the question of the validity of a moral code is quite independent of the question of its origin. This does not seem to me to be true."[16] He holds that an "inquiry into the origin of a judgement may have the effect of establishing its validity." His example is a geometrical one: the judgement that the sum of the angles of a triangle are equal to two right angles. The "historical origin" of this judgment lies in its premises, plus the activity of inferring. If the premises were actually instances of knowing, and if the inferring was "the apprehension of a necessary connexion," then "our inquiry into the origin of the judgement in question will have established its validity." Some parallel cases might be found in ethics.

In practice, naturalists are likely to use a genetic method in regard to ethical views which would be difficult to support or to challenge in a purely formal way. Let us consider the following argument, which is similar to some actually advanced:

"Zulus are inferior to Europeans in culture, intelligence, and morality; therefore, white Europeans should rule them for the Zulus' own good."

Let us also assume that the argument could be made logically valid. Now it might be difficult to demonstrate directly and incontrovertibly that the Zulus are not inferior to whites. It could probably be demon-

strated that the Europeans find it advantageous to rule the Zulus, however. An inquiry into social and economic questions here could lead a person to deny the inferiority of the Zulus, or at least to suspect that reports of their inferiority were biased because of socio-economic factors. Hence he may come to doubt the truth of the conclusion.

The matter can be put in another way: in many areas, although one may well hold that the truth or falsity of a statement is objective, it may be difficult to get evidence which would directly show whether the statement is true or false. A genetic account which emphasizes social and historical factors could then be very useful in arriving at a conclusion about the truth or falsity of the statement in question. To insist that one stay on a formal level does not always seem wise.

It may be helpful to distinguish, in this context, between what *actually is* objective truth, genuine evidence, and validity, and what *is accepted* in a given time and place as truth, evidence, or validity. It seems tempting to many to assume that what a given culture or subculture takes as true actually is true, in ethics and in other areas.

Some of the questions involved can be illustrated by the following comments of H. Dingle on the relation of science to its social setting:

> "Take any scientific law you like . . . they are true or false apart altogether from any consideration of social conditions or anything else except the uncontrollable facts of nature and the unavoidable requirements of logical necessity."

> ". . . forms of social organization can at most affect the rate of [scientific] progress, the place where the discoveries are made, and perhaps in very minor details the order in which they are made. On the main course of scientific development, their influence is non-existent."

> ". . . the actual evidence on which [scientific facts and theories] rest . . . in the great majority of sciences has nothing to do with the time, place, and circumstances of their discovery or invention . . ."[17]

Even in terms of Dingle's discussion, he does not seem to be quite correct. If a wide variety of human cultures are considered, surely cultural factors do affect the "main course of scientific development." And if the earliest beginnings of a science are kept in mind, the social situation would be quite important in determining which sciences developed. D.

Struik gives some interesting examples of the relation of social processes to mathematics.[18] The Hopi did not develop nuclear physics. The Greenland Eskimo were hardly likely to develop a scientific psychology. Dingle perhaps would restrict his comments to rather highly developed cultures; and his view of science to advanced sciences.

More important, however, is that what *is believed* to be criteria for truth or falsity, and what *is accepted* as "actual evidence," or as "uncontrollable facts of nature," or as "logical necessity," does depend partly on social conditions — on the time, the place, and the circumstances. The forms of social organization may determine to a considerable extent what areas are thought to be amenable to a scientific treatment. In our culture, contemporary philosophers tend to regard scientific psychology with approbation, but some Indian philosophers may feel strongly that such a psychology cannot explain human behavior.[19] Surely genetic factors are relevant here. A naturalist can hold that truth is objective, but at the same time regard as philosophically important what given individuals, in given contexts, *believe* is objectively true. The modern scientist's view of what constitutes genuine evidence may be quite different from the views accepted when science was in its infancy. Present views may be modified in the future.

To summarize: The thesis of this paper has been that although there are several versions of "the" genetic fallacy which are genuine fallacies, a naturalistic ethics need not commit them. It has been argued that in several ways a genetic method can be pertinent to ethics. As was suggested, it may be suspected that some writers are opposed to a scientific and developmental approach to ethics, and that this has led them to raise charges of the genetic fallacy in order to undermine such endeavors. One author has gone so far as to say that the attempt "to apply the canons of scientific predictability to moral phenomena is but a particular manifestation of the genetic fallacy."[20] While a naturalist could confuse the truth, the validity, or the meaning of an ethical statement with the genesis and development of that statement, there is nothing in the naturalistic approach to ethics which necessitates that confusion. What the naturalist often argues is that the history and the cultural context of an idea has some bearing on our evaluation of it. Even if the naturalist is incorrect here, his error is not that of committing the genetic fallacy.

NOTES

[1] Felix Kaufmann, *Methodology of the Social Sciences* (New York, Oxford, 1944), p. 16.

[2] Gustav Bergmann, "Logical Positivism", *A History of Philosophical Systems*, ed. Vergilius Ferm (New York, Philosophical Library, 1950), p. 482, note 7; Melvin Rader, *Ethics and Society* (New York, Holt, 1950), p. 50.

[3] Robert K. Merton, "The Sociology of Knowledge", *Isis*, XXVII (1937), p. 493.

[4] Ledger Wood, entry under "genetic fallacy", *The Dictionary of Philosophy*, ed. Dagobert Runes (New York, Philosophical Library, 1942).

[5] John H. Randall, Jr., "History and the Social Sciences," *Readings in Philosophy of Science*, ed. Philip P. Wiener (New York, Scribner's, 1953), p. 324.

[6] Kaufmann, *op. cit.*, p. 16.

[7] Bergmann, *op. cit.*, p. 482, note 7.

[8] A. C. Ewing, *The Definition of Good* (New York, Macmillan, 1947), p. 55.

[9] Abraham Edel, "Context and Content in the Theory of Ideas," *Philosophy for the Future*, ed. Roy W. Sellars, V. J. McGill, and Marvin Farber (New York, Macmillan, 1949), p. 426.

[10] Hans Freistadt, "Dialectical Materialism: A Friendly Interpretation," *Philosophy of Science*, 23 (1956), p. 109.

[11] Randall, *op. cit.*, pp. 323–24.

[12] Thomas H. Huxley and Julian S. Huxley, *Touchstone for Ethics* (New York, Harper, 1947), pp. 149–50; 195–96.

[13] *Ibid.*, p. 149.

[14] Abraham Edel, "Some Trends in American Naturalistic Ethics," *Philosophic Thought in France and The United States*, ed. Marvin Farber (Buffalo, University of Buffalo, 1950), p. 589.

[15] In view of Edel's general thesis, it is interesting that in a note to a reprinting of "Ethics as Pure Postulate," Williams said that he soon found "intolerable its flavor of voluntarism and subjectivism." *Readings in Ethical Theory*, ed. Wilfrid Sellars and John Hospers (New York, Appleton-Century-Crofts, 1952), p. 665.

[16] William D. Ross, *The Right and the Good*, (Oxford, Clarendon Press, 1930). The quotations in this paragraph are found on p. 14. In *Foundations of Ethics* (Oxford, Clarendon Press, 1939), he repeats on p. 16 the argument of his earlier book.

[17] Herbert Dingle, "History of Science and the Sociology of Science," *The Scientific Monthly*, 82 (1956), p. 109, 111.

[18] Dirk J. Struik, "Mathematics", *Philosophy for the Future*, ed. Sellars, McGill, and Farber.

[19] For example, the remarks on psychology by Bhikhan Lal Atreya, "Indian Culture, Its Spiritual, Moral, and Social Aspects," *Interrelations of Cultures* (Paris, UNESCO, 1953), p. 129.

[20] William F. Quillian, *The Moral Theory of Evolutionary Naturalism* (New Haven, Yale, 1945), p. 99.

NOTES ON THE APPLICATION OF FORMAL
METHODS IN THE SOFT SCIENCES

by

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I.   Mathematical logic (or symbolic logic) has proved to be an indispensable tool for investigations in the foundation of mathematics, but an equal success of application in what may be called the "soft" sciences, such as the social sciences, the study of norm systems etc., is not apparent. These are some notes, programmatic in character in that they give suggestions and outlines instead of supplying well-developed theories, on the application of formal methods in the non-exact sciences.

Within the exact non-formal sciences (i.e. "universal" physics) well-established formal methods are in use, and the worker in the field makes every effort to master the formal apparatus before attacking his problem field proper. And the formal methods are essential to him. The soft sciences appear to be in need of suitable formal methods; but what the formalist so far has offered does not seem to have been very successful, and the use of such methods is not extensive. Some remarks on the cause of failure will be offered. (As is already apparent, we do not in this paper take into consideration formal methods from mathematics proper, but methods usually placed within the field of symbolic logic.)

Skjervheim, in his *Reason in Society and Modern Logic*, (this journal, vol. 1, no. 4), voices some critical remarks on the application of formal logic in the social sciences, and as regards what has been achieved so far (with the idea of total application), it may be justified. Skjervheim appropriately points out the need of non-extensional operators; but one may suspect that he is prejudiced by the view of Carnap and the latter's inclination to explain away the modalities and other non-extensional operators by use of the meta-language. However, it should be perfectly

feasible to construct a language in which iteration of admissible operators is allowed within that system (in fact, most modal logics yet given so permit). Thus there appears to be no compelling reason to return to something like the logic of Hegel, and so far there should be no despair of failure at applications. Symbolic logic treats sets of statements; why should not sets of statements taken from the field of social sciences be studied by means of formal techniques? It is realized that the difficulty of hitting upon the most promising abstractions and simplifications concerning the various non-extensional operators currently in use is vastly greater than handling the extensional language of mathematics, but it is my firm belief that the method this paper tries to represent offers a fruitful approach.

As a first cause of failure at application we may mention the pretentious program of *total application*, i.e. of constructing a universal language sufficient as a framework for all scientific work. But then we stand the risk of giving a mere transcription of results already obtained, and we have to answer the charge of barrenness. Compare the situation in mathematics at the turn of the century. The *Formulaire* of Peano and his collaborators supplied hardly more than a translation of previously known results, so Poincaré rightly leveled the charge of unfruitfulness of the approach, since it made mathematics clumsy and unmanageable, without increasing the body of results. But then results, rather unimaginable without the technique of mathematical logic were found; it suffices to mention the famous Gödel incompleteness theorems. A Gödelian period in the application of symbolic logic to the soft sciences has not yet arrived at, in spite of interesting efforts of Hull and Fitch in application within the psychology of rote learning, and Woodger within biology.

Following the vision of total application, a second cause is the resulting heaviness in the formal apparatus, making it so complicated that it proves unmanageable for the non-formalist; at least he has to be very adept in formal manipulations if anything but trivial results are to be the outcome. Formal apparatus intended as a total framework for a problem field (of sufficient "strength") is non-constructive, e.g. the derivation of consequences from a given assumption set requires mathematical invention, and such a need on the part of a user, a non-formalist, seems to be a further cause of failure in application.

The above is not, of course, an argument against a theoretical possibility of the program of total application. It is an argument based on practical considerations against the fruitfulness of such an approach, at

least in the present situation. If formal methods applicable within the soft sciences are to be constructed, they must be so constructed that they are of use to the non-formalist, escaping the fate of being ignored.

These preliminary remarks, claiming no more than to give our impression of the situation, lead naturally to consideration of a *criterion of simplicity* as regards the application of symbolic logic in the soft sciences. The goal is always to arrive at interesting results with simple and easily operated apparatus, but here a dilemma is encountered that is perhaps non-escapable. It may be described as the balancing act between *adequacy in description* of the problem field and the *simplicity in formal methods*. If it were necessary to construct methods giving a sufficiently detailed description of everything relevant within a specified problem field, the resulting apparatus, were it constructable, would surely be too complicated for the non-formalist (and perhaps for the formalist too). If due considerations were to be given to simplicity and easy manageability, the problems to which the apparatus would be applicable might be rather trivial in character.

In the present situation this balancing act dilemma seems to be unavoidable if the program is total symbolization within the problem field, but it may perhaps partially be resolved by restricting attention to what can be called *local application* of symbolic logic. I feel it is more fruitful not to attempt a construction of a total framework, i.e. a universal language for a soft science, but to single out, within the broader context, problems of detail concerning deductive properties to which symbolic methods may be applied with satisfying result. Problems of the type: Is this sentence derivable from this assumption set? Is this set of sentences compatible? etc., are of local nature, i.e. they may be decided by formal methods without the need of total symbolization of the problem field. A partial application of symbolic logic to exactly the set of relevant sentences, being usually a "small" subset of all the sentences pertaining to the total problem field, suffices, hence more simple methods may be used than in a total field application. A thesis of this paper is that more effort should be spent on constructing methods applicable to local problems within a broader field than on contemplating a universal language.

Here it should be noted that these remarks are heuristic. The discussion proceeds on an intuitive level, and it is rather difficult to raise the level of preciseness, because our initial problem itself is by no means precise; thus any effort to remove every vagueness and ambiguity has to include a precization by definition, and then the relevance of the

resulting problem to the initial one can be questioned, and we are involved in semantical considerations of empirical content. So we let these remarks remain rather loose and heuristic, though some progress can be made toward singling out features not incompatible with a simplicity requirement.

1. *Sparseness in formal symbols,* i.e. much constraint should be shown in introducing new symbols, whether within a calculus or in the meta-language. A long list of definitions each either introducing a new symbol or a new technical term can be tedious and confusing.

2. *Simplicity and perspicuity in rules of deduction.* Proofs within a formal calculus are usually long and tedious; thus derivation rules in the meta-language, paralleling intuitive reasoning, should be established as soon as possible in the construction of a formal language. The formal rules of deduction should also be given as intuitive a structure as possible (cf. the Gentzen-type systems of logic). When the systems are decidable, the mechanical decision rules should be systematized so as to be applicable in practical derivations; further, the exploration of decidable sub-systems of a given calculus should be carried farther than is commonly done · (e.g. the introduction of restricted quantifiers within the quantificational calculus is sufficient for many applications within soft sciences). This requirement is in some way connected with the problem of giving well-motivated choices of axiom systems.

3. *No great demands of mathematical invention* on the part of the user should be required. Given an undecidable system of mathematical logic, it is a non-effective problem, requiring mathematical invention, to derive consequences from a specified set of assumption formulas. To "see" possible consequences of an assumption set demands mathematical intuition. These difficulties seem very real to the non-formalist when he attempts to apply non-constructive systems of mathematical logic.

Before proceeding with the discussion it should be mentioned that the features given above of a possible simplicity criterion are not independent, nor need they be compatible in every case, the Sheffer stroke being an example of extreme sparseness in formal symbols but corresponding complications in proofs.

Essential to a criterion of simplicity seems to be a *requirement of effectivity,* i.e. of effective decidability of "admittable" problems in connection with the calculus, e.g. proofs of existence should in some way be concealed instructions of constructions to be completed in a finite number of steps. It would be a desirable feature of a formal

system if, e.g., effective decidability could be obtained of a problem of the type: Is this sentence a consequence of that assumption set? Working with the idea of total application, we have to abandon this requirement of effectiveness as soon we start using the full predicate calculus (provided 'effectivity' is construed as usual in terms of general recursiveness). Working with the idea of local application, effective decidability may be obtained in problems which turn non-effective when considered within the context of total application. It may be mentioned that a requirement of effectivity is of the greatest importance to the practical working physicist in regard to, e.g. the solution of differential equations. It is of no use to him that a pure mathematician has given an existence proof by non-effective methods because he wants the actual solution. The same applies to our "soft" scientist; given a problem, he wants methods which give him the solution, not merely the knowledge that a solution exists.

A requirement of effectivity is not independent of the three points given above, e.g. if we were to make precise the third feature about the non-requirement of mathematical invention in the use of some symbolic calculus, we would probably agree that a method required no use of mathematical invention if it was decidable, because then for each type of problem a uniform method of solution applicable to every particular instance could be described, eliminating the need of any mathematical invention.

The partial criteria outlined above, i.e. the sparseness of formal symbols, simplicity and perspicuity in deduction rules, no demand of mathematical invention and a requirement of effective constructions, may be combined into a demand that symbolic methods for application in the soft sciences should preferably be *algorithmic* in character. Here it suffices to say that a problem type can be algorithmically solved if there is given a uniform applicable method of solution which for every specific problem yields an answer in a finite number of symbol manipulations. As an example, the sentential calculus is algorithmic in character (is a calculus with an associated algorithm), e.g. to solve the problem if A L-implies B, the usual truth table methods give a uniform method of problem solution for every choice of A and B. If we were going to introduce a concept 'algorithm', we should have to utilize the concept of general recursiveness, thus arriving at the result that a certain calculus is effectively decidable if and only if there constructively can be given an algorithm for the system. (So it appears that the criteria of effectivity and associated algorithm are equivalent (by precization proceeding by

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definition), and if a formal method is effective, then the three points given above are satisfied, except possibly the first, which, however, is of the least importance.) It is now obvious that if we may associate a truth table method with a symbolic calculus, we have an algorithm for that calculus, and in this connection it is asserted that the formal system satisfies any reasonable criterion of simplicity (because then in operating the formal calculus only ability to follow predescribed rules is required).

Thus the task is to explore extensions of truth value methods.

II. Knowledge of the sentential calculus will be assumed, i.e. of truth table procedures and normal form transformations as contained in, e.g. Hilbert, Ackermann: *Principles of Mathematical Logic,* or a similar text. The sentential calculus is supposed to be constructed in the classical manner, and to a sentential calculus with n sentential variables (called Sk_n) we introduce a class of value-series constructed out of the letters 't' and 'f', each series having a length of 2^n. The correspondence rule is:

Rule A: The value series corresponding to α_i (the i'th sentential variable) within Sk_n is $2^{i-1}(2^{n-i}t\ 2^{n-i}f)$, i.e. 2^{n-i} t's followed by 2^{n-i} f's, the whole repeated 2^{i-1} times. This is written

$$2^n V \alpha_i = 2^{i-1}(2^{n-i}t\ 2^{n-i}f).$$

By the usual associated truth value tables of the sentential connectives every admittable (classical) Sk_n-expression is mapped onto one and only one value series, but different Sk_n-expressions may be mapped onto the same series. To a given value series thus corresponds a class of Sk_n-expressions (all being L-equivalent), hence the mapping introduced gives a partition of the set Sk_n into equivalence classes (2^{2^n} in number). In our further reasoning we will work with equivalence classes but use a terminology as if we employed the Sk_n sentences themselves; thus we say that A L-implies B, i.e. the equivalence class, which A represents, L-impl. the class of B, or equivalent, that every L-equiv. expression of A L-impl. every L-equiv. expression of B. The usefulness of this convention is to make the universe Sk_n consist of a finite number of sentences, each being inequivalent to the others (thus, e.g. given A as an Sk_n-expression, in our terminology it has only a finite number of consequences each L-inequiv.; in the usual textbook terminology we have to admit an enumerable infinity of consequences).

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To proceed with the extension of common truth table methods, re-stricted quantifiers are introduced.

(1) $(x)F_s x =_D F_s a_1 \ \& \ F_s a_2 \ \& \ \ldots \ \& \ F_s a_n.$

(2) $(Ex)F_s x =_D F_s a_1 \ v \ F_s a_2 \ v \ \ldots \ v \ F_s a_n.$

The domain of individuals (a_i) is assumed finite and non-empty, con-taining $n \geq 1$ individuals. To obtain a value series correspondence we introduce by definition the mapping:

Def. A: F_{sa_p} is mapped onto $\alpha_{(s-1)n+p}.$

Def. A in conjunction with (1) gives

(3) $2snV(x)F_s x = 2snV\alpha_{(s-1)n+1} \ \& \ \ldots \ \& \ 2snV\alpha_{sn} =$
                $2(s-1)n(t \ (2n-2)f \ f).$

Analogous with the existential quantifier:

(4) $2snV(Ex)F_s x = 2(s-1)n(t \ (2n-2)t \ f).$

A set of admittable classical expressions is now introduced, the re-stricted pure quantificational calculus, qF_m, with m predicate variables, generated from the atomic quantificational expressions '$(x)F_i x$' and '$(Ex)F_i x$' (with $1 \leq i \leq n$) by use of the sentential connectives. By an obvious induction proof we can establish the adequacy of the following rule of value series reduction (so that with the reduced series-length every qF_m-expression corresponds to one and only one value series; qF_m-expressions which before the reduction corresponded to different series correspond to distinct series after the reduction, and to each reduced series correspond a set of L-equiv. qF_m-expressions, the parti-tion of the set qF_m being the same after as before the value series reduction):

Rule B: In the value series expressions of length 2mn (i.e. series cor-responding to qF_m) each occurrence of the number 2n is to be replaced by 3, such that (i) 2n is replaced by 3, (ii) 2n—2 is replaced by 1, and (iii) 2pn is replaced by 3p.

If we use 'qsV' for the set of reduced value series corresponding to qF_s, '$F(F_s)$' for a classical expression with only the predicate vari-

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able 'F_s', we get as some typical coordinated value series (using 'v_{sj}' for either 't' or 'f'):

(5) $\qquad\qquad qsV(x)F_s x = 3s\text{-}l(tff)$.

(6) $\qquad\qquad qsV(Ex)F_s x = 3s\text{-}l(ttf)$.

(7) $\qquad\qquad qsVF(F_s) = 3s\text{-}l(v_{s1}v_{s2}v_{s3})$.

(8) $\qquad\quad q(s+k)VF(F_s) = 3s\text{-}l(3kv_{s1}\ 3kv_{s2}\ 3kv_{s3})$.

(9) $\qquad\quad qmVF(F_s) = 3s\text{-}l(3m\text{-}s\ _{s1}\ 3m\text{-}sv_{s2}\ 3m\text{-}sv_{s3})$.

Thus the problem of giving an algorithm associated with the calculus qF_m is solved, and we turn to a combined calculus of qF_m and Sk_r, with the set of atomic expressions '$(x)F_ix$', '$(Ex)F_ix$' and 'p_q' (with $1 \leq i \leq n$, $1 \leq q \leq r$), and set of admittable expressions generated from the atomic ones in the usual way by the sentential connectives (giving a calculus $qF_m + p_r$). We complement def. A by:

Def. A': p_q is mapped onto α_{mn+q}.

Utilizing this mapping, we may establish the adequacy of this reduction rule:

Rule C: In the value series expressions (of length $2mn+r$) corresponding to the calculus $qF_m + p_r$, at each occurrence the number $2tn+v$ is to be replaced by $3t2v$.

To escape possible ambiguities we require that the cardinality of the domain of individuals is greater than r (but still finite); this hardly constitutes any restriction in practical applications of the calculus. As some typical coordinated value series we obtain:

(10) $\qquad qm + rV(x)F_s x = 3s\text{-}l(3m\text{-}s2rt\ 3m\text{-}s2rf\ 3m\text{-}s2rf)$.

(11) $\qquad qm + rV(Ex)F_s x = 3s\text{-}l(3m\text{-}s2rt\ 3m\text{-}s2rt\ 3m\text{-}s2rf)$.

(12) $\qquad\qquad qm + rVp_q = 3m2q\text{-}l(2r\text{-}qt\ 2r\text{-}qf)$.

Hence we have an algorithm associated with the calculus $qF_m + p_r$ by use of (10)—(12) and the truth tables associated with the sentential connectives. Yet we have strictly observed a requirement of simplicity, but what about the adequacy in description? Triviality may perhaps be escaped by indicating two further extensions.

Turning to the monadic quantificational calculus, we give the follow-

ing two formal expressions the status of being L-true (they may be obtained by suitable extensions of def. (1) and (2)):

(13)   '(x) (Ax & Bx) . ∞ . (x)Ax & (x)Bx' is L-true.

(14)   '(Ex) (Ax v Bx) . ∞ . (Ex)Ax v (Ex)Bx' is L-true.

Here 'Ax' and 'Bx' are any compound predicate expressions with x as only free variable. Then we admit as classical expressions the full monadic quantificational calculus, generating a universe $q(F_1, \ldots, F_m)$, if the maximal number of predicates is m. An elementary quantificational expression has the form '(x)$(F_1, \ldots, F_m)$' or '(Ex)$(F_1, \ldots, F_m)$' where the matrix (or operand) '$(F_1, \ldots, F_m)$' is constructed from expressions '$F_i x$' $(1 \leq i \leq m)$ by use of the sentential connectives. The above indicated extension of def. (1) and (2) may be used to show that every admittable expression in $q(F_1, ., F_m)$ can be written as a truth function of elementary quantificational expressions (e.g. '(x) (Ax v (y)By)' is L-equiv. '(x)Ax v (y)By', etc.).

As a first step in constructing a mapping onto a set of truth value series we describe a procedure which effectively coordinates to any admittable $q(F_1, ., F_m)$-expression a certain *standard form*, the given expression and the standard being L-equivalent. First every universal quantifier is rewritten as an existential by use of: '(x)Ax' is L-equiv. '—(Ex)$\bar{A}$x'. Then every matrix is transformed to the principal disjunctive normal form, thence we use (14) in reducing the scope of each quantifier as much as possible. The resulting expression we call the standard form associated to the given expression and it is a truth function of elementary expressions of the type:

(15)   (Ex) $(F_1^+x \& F_2^+x \& \ldots \& F_m^+x)$,

Where 'F_i^+x' is either '$F_i x$' or '$\bar{F}_i x$'. Now we can conceive of each matrix of type (15) as a new predicate variable P_j, hence the original m predicates F_i gives 2m predicates P_j. Next we introduce a concept of *associated index set* to a matrix of type (15). To a matrix '$F_1^+x \& \ldots \& F_m^+x$' we associate the index set $(i_1, i_2, \ldots, i_m)$ where $i_k = 0$ if 'F_k^+x' is '$F_k x$', and $i_k = 1$ if 'F_k^+x' is '$\bar{F}_k x$'. Utilizing this concept we may state the following rule, of some importance if viewed as constituting a part of the associated algorithm to $q(F_1, \ldots, F_m)$:

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Rule D: Given the matrix '$F_1^+ x \, \& \ldots \& \, F_m^+ x$' with associated index set $(i_1, \ldots, i_m)$, this matrix is to be replaced by the predicate P_j, with j determined by

$$(16) \qquad\qquad j = 1 + \sum_{k=1}^{m} 2k{-}1 i_k.$$

Hence any admittable $q(F_1, \ldots, F_m)$-expression is mapped onto a P_j-expression, called its associated *P-standard form*, such that the given expression and the P-standard form expression are declared to be L-equivalent. (It should be noted that an inverse of rule D is easily given.) To complete the value series coordination it is sufficient to consider the elementary P_j-expressions. Here we stumble on an important restriction. Whereas the calculi Sk_n, qF_m and $qF_m + p_r$ are *free* calculi (in the mathematical sense as in 'free group' etc.), which signifies that no restriction need be placed on the value series mapping, we have, when considering the set of P_j-expressions, a *defining relation*, if the adequacy of the mapping is to be secured. This relation is:

(17)   '$(Ex)P_1 x \, v \, (Ex)P_2 x \, v \ldots (Ex)P_{2^m} x$' is L-true.

Then we may complete the mapping construction by the rule:

Rule E: The expression '$(Ex)P_j x$' is mapped onto the value series $(2j{-}1{-}1 \; (2^{2m}{-}jt \; 2^{2m}{-}jf)) \; (2^{2m}{-}jt \; (2^{2m}{-}j{-}1)f)$.

This seems more complicated than it really is. What rule E expresses is that the series coordination to the elementary P_j-expressions is the same as the series coordination within the sentential calculus Sk_{2^m}, except that the last entry in the series is omitted on account of the defining relation (17). Rules D and E have to be supplemented to take care of the case that a $q(F_1, \ldots, F_m)$ matrix is contradictory, if so happen we determine that the associated P-standard form shall be '$-((Ex)P_1 x \, v \ldots v \, (Ex)P_{2^m} x)$'. By annexing this rule we have completed associating an algorithm to the calculus $q(F_1, \ldots, F_m)$, giving a mapping of the set of admittable expressions onto the set of value series of length $2^{2m}{-}1$ (, and working with equivalence classes the inverse mapping may also easily be constructed as giving, as in the other calculi, a one-to-one correspondence between equivalence classes (after the relation of L-equiv.) of calculus expressions and value series). Further we may

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note that the calculi qF_1 and $q(F_1)$ have the same set of classical expressions, but that we have constructed two different mappings. However, they are easily seen to be equivalent, e.g. the corresponding value series length is in both cases 3 equals 3^1 and also $2^{2^1}-1$, but the "internal" order of t's and f's in the value series is not the same, which is of no importance.

As a last example of extensions of truth table methods we describe a simple system of quantificational calculus containing only twoplace predicate variables. To obtain a compact description of the elementary expressions, we introduce some abbreviations: 'qx' for either of the quantifiers and 'b' for the corresponding sentential connective to a quantifier (i.e. 'b' is '&' if 'qx' is '(x)'). Then we have the definitions:

$$(18) \qquad qxqyFxy =_D qyFa_1y \cdot b \cdot qyFa_2y \cdot b \cdot \ldots \cdot b \cdot qyFa_ny,$$

where for every i (with $1 \leq i \leq n$)

$$(19) \qquad qyFa_iy =_D Fa_ib_1 \cdot b \cdot Fa_ib_2 \cdot \ldots \cdot b \cdot Fa_ib_n.$$

Similarly for the expression type 'qyqxFxy'. Then we use this set of elementary quantificational expressions as atomic signs for a calculus bF_1, the calculus being generated from the initial set by the sentential connectives. The problem is, as usual, to define a map onto a set of value series expressions.

As before, we introduce a mapping definition:

Def. B: Fa_ib_j is mapped onto α_p, where $p = j + (i-1)n$.

Here p is seen to vary between 1 and n^2 (n being the number of individuals in the considered domain), thus we get a series length of $2n^2$.

From the definitions and the sentential calculus we easily obtain the following deductive hierarchy:

$$(20)$$
$$(x)(y)Fxy \text{ L-impl.} \begin{cases} (Ex)(y)Fxy \\ \qquad\qquad \text{L-impl.} \\ (Ey)(x)Fxy \end{cases} \begin{cases} (y)(Ex)Fxy \\ \qquad\qquad \text{L-impl.} \quad (Ex)(Ey)Fxy \\ (x)(Ey)Fxy \end{cases}$$

A radical reduction in series-length is established by the following rule, given in table form:

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Rule F: The mapping resulting from def. B can be adequately described by the given truth value series coordination to the elementary expressions within bF_1 (in conjunction with the truth tables associated with the sentential connectives):

	1	2	3	4	5	6	7	8	9	10	11
(x)(y)Fxy	t	f	f	f	f	f	f	f	f	f	f.
(Ex)(y)Fxy	t	t	t	t	f	f	f	f	f	f	f.
(y)(Ex)Fxy	t	t	t	t	t	f	t	f	t	f	f.
(Ey)(x)Fxy	t	t	f	f	t	t	f	f	f	f	f.
(x)(Ey)Fxy	t	t	t	f	t	t	t	t	f	f	f.
(Ex)(Ey)Fxy	t	t	t	t	t	t	t	t	t	t	f.

The reduction of value series length is from $2n^2$ to 11, greatly increasing the ease of manipulations within the series universe. Rule F may be established by proving the assertions (by use of (18), (19), (20) and def. B): (I) Every value distribution in rule F is realizable (granted an obvious concept 'realizable') within univ. $2n^2V$ for $n \geq 3$. For $n = 2$ the distributions in lines 3, 5, 8 and 9 is not realizable. (II) Conversely we see that no other value distributions than those given in rule F is realizable ($n \geq 3$). — (I) and (II) in conjunction establish the adequacy of the truth table given in rule F (if $n \geq 3$, a restriction of little practical importance). As with the other calculi we may, if working with equivalence classes of bF_1-expressions, expand the above mapping into a one-to-one correspondence, thus securing an algorithm to the calculus bF_1 (easily extendable to bF_m for any m).

As regards the illustrative constructions outlined in this note, we remark that there has been no relaxing in the requirement of giving an algorithm in connection with the different calculi, thus problems of, e.g., derivability may be decided in a finite number of symbol manipulations. Further, the constructed mappings of the calculi onto certain sets of value series is one-to-one working with equivalence classes. We thus have an effective procedure given a value series to find a classical expression whose map is the given series (the inverse mapping may be constructed by using the concept of normal form, i.e. first giving classical expressions whose coordinated series contains only one occurrence either of 't' or of 'f').

Thus we may assert that the above constructions are in accordance with a requirement of simplicity in formal apparatus (especially if we give meta-theorems to simplify the value series coordination). But what

about adequacy in description? This question is difficult to answer; only attempts at application may give an indication. But it seems reasonable to believe, mindful of the idea of local application, that by iterations and combinations of the construction types used above, sufficient richness in expressive means may be obtained to avoid mere triviality in problem description (compare note IV).

Concerning the extension methods we observe that a mapping of an extended calculus is obtained by first mapping it into a sufficiently rich sentential calculus (i.e. constructing it within a calculus Sk_m for sufficient large m). This method furnishes a basis for general assertion of effective decidability of problems pertaining to the extended calculi, because every such problem is mapped onto a problem pertaining to the sentential calculus, and the sentential calculus is effectively decidable. What we have done is to reduce the extended calculi to certain systems of sentential calculi (i.e. to certain sentential calculi with additional defining relations), a method of problem-solving not uncommon within mathematics.

We have for each extended calculi required the domain of individuals to be of finite cardinality, but then in connection with the calculi we have established rules of series reduction in such a way that this cardinal number is irrelevant for the reduced calculus-onto-series mapping. We have:

(i)   the qF_m-reduction is from $2mn$ to $3m$.

(ii)   the $qF_m + p_r$-reduction is from $2mn+r$ to $3m2r$.

(iii)   the length of $q(F_1, ., F_m)$ is directly obtained as $2^{2m}-1$.

(iv)   the bF_m-reduction is from $2mn^2$ to $11m$.

These reduction results may be used to abandon the requirement of a finite cardinality of the individual domains, i.e. to introduce full quantification within the considered calculi and yet retain the property of effective decidability. More precisely, we may introduce calculi qF'_m to bF'_m admitting individual domains of arbitrary cardinality giving the above derived value series coordinations as a coordination by definition, regarding the reduction proofs as proofs of compatability in case of individual domains of finite cardinality. This extension may be considered as a (heuristic) argument in favor of the above-expressed belief that we approach calculi which, while satisfying the requirement of simplicity, supply expressive means securing adequacy in description. (The subject matter of this note (and note VI) is more extensively treated in a forthcoming text (in Norwegian) by Professor Næss and the author).

For expedience in the following notes we introduce a concept of *calculus of type K,* thereby meaning a calculus for which a mapping is constructed onto a well-determined set of value series, giving a one-to-one correspondence between equivalence classes within K (from the relation of L-equivalence) and value series within the specified series universe, such that when the number of variables (whether sentential or predicate) is fixed, series of uniform length shall result by the mapping. The above constructed calculi are all calculi of type K.

III. Some calculi constructions have been outlined; the purpose of this note is to acquire some insight into what problems may be treated by these calculi. We shall be concerned with what may broadly be described as deductive properties of sets of statements.

One problem type concerns the compatability of sets of sentences, i.e. given a set of sentences (A_i), may we assert every A_i without being involved in any logical falsehood? (Here we assume the process of symbolization to be completed, semantical considerations to be reserved for note VI.)

Another problem type turns on the notion of deducibility, i.e. given a sentence A_k, is it deducible from the set (A_i), and further, what are the consequences of the set (A_i)?

Problems of this type may be treated within the framework supplied by the theory of deductive systems, of which we give a brief outline in this note. (The mathematician will notice that the procedure here is merely to transcribe a certain part of ideal theory in finite Boolean algebras; thus this note is entirely expository, but is appended because of its value as an argument in connection with our discussion of a simplicity requirement and its value in the study of norm systems, note IV).

In the rest of this note we assume that every sentence A_i belongs to a calculus of type K; then the definition of a deductive system may be introduced:

Def. A_1: A *deductive system* is a set of sentences (A_i) satisfying the properties:
   a. If A_i and A_j are contained in the set, then also $A_i \& A_j$.
   b. If A_i is contained in the set and A_i L-impl. A_j, then A_j belongs to the set.

We use the letter 'D' as a designatum for a deductive system, the symbols 'ε', 'ℂ' etc. in the usual set-theoretic significance. Then a. and b.

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may be rewritten: a. $A_i \epsilon D$ and $A_j \epsilon D$, then $A_i \& A_j \epsilon D$, b. $A_i \epsilon D$ and A_i L-impl. A_j, then $A_j \epsilon D$.

Def. A_2: A deductive system D is *proper*, if and only if there exists no A_i such that both $A_i \epsilon D$ and $\overline{A_i} \epsilon D$.

A deductive system D is *pseudo*, if and only if for all A_i so is $A_i \epsilon D$.

A deductive system D is *empty*, if and only if $A_i \epsilon D$ if and only if A_i is L-true.

Some simple theorems yield a new description of proper and pseudo deductive systems. (Note, working with the concept of equivalence classes of expressions within K we do have the L-false sentence).

(1) A deductive system D is proper, if and only if there exists one $A_i \overline{\epsilon} D$.

(2) A deductive system D is pseudo, if and only if the L-false sentence is contained in D.

As an example of the proof method, arising from the underlying calculus type K, we prove (1): If D is proper, then at most one of the pair A_i, $\overline{A_i}$ (for any i) is contained in D, thus there exists at least one $A_j \overline{\epsilon} D$. Conversely, if there exists at least one $A_j \overline{\epsilon} D$, then the L-false sentence is not in D, if so, then $A_j \epsilon D$ because the L-false sentence L-impl. A_j (def. $A_1 b$.). But when the L-false sentence is not in D, then there is no pair A_i, $\overline{A_i}$ in D, because if there were, $A_i \& \overline{A_i}$ would be in D (def. $A_1 a$.), but $A_i \& \overline{A_i}$ is the L-false sentence; hence the theorem follows by intuitive reductio ad absurdum.

The proof method used here is typical of modern algebra, establishing the desired existence but not giving an effective method of construction. As we are working with a calculus of type K, we may give the proof directly as follows (for convenience using the below introduced concept of system generator): If D is proper and A is the generator of D, then $\overline{A}$ is not in D (here $\overline{A}$ can be constructed effectively). Conversely suppose a A_j not ϵD, and consider any pair A_i, $\overline{A_i}$, we shall show that not both are in D. From the fact that A_j is not in D follows that the generator A of D is not the L-false sentence, hence the value series S_A corresponding to A has at least one t. Consider the series S_{A_i} and $S_{\overline{A_i}}$ corresponding to A_i and $\overline{A_i}$; where S_A has t one of these has f (which one may effectively be decided). Suppose it is S_{A_i}, then A

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does not L-impl. A_i, and because A L-impl. every A_k in D, A_i is not in D. We see that the requirement that at least one $A_j \varepsilon D$ is used to assert that the generator A of D is not the L-false sentence, thence it can be effectively decided for every pair A_i, $\overline{A}_i$ which one is not contained in D (or possibly if neither is in D). We have elaborated on this proof to illustrate how the truth value algorithm associated with the calculus K gives effectivity in proof constructions.

The list of definitions is continued:

Def. A_3: Given a deductive system D: A is a *generator* of D, if and only if $A \varepsilon D$, and for every $A_i \varepsilon D$, then A L-impl. A_i.

Given deductive systems D_1 and D_2:

D_1 is a *supersystem* of D_2, if and only if $D_2 \subset D_1$ (i.e. for every $A_i \varepsilon D_2$, so $A_i \varepsilon D_1$).

D_1 is a *subsystem* of D_2, if and only if $D_1 \subset D_2$.

The list of theorems is continued ((3) is proved without the use of either (1) or (2) as necessary for the constructive proofs of (1) and (2) as given above).

(3)   Every deductive system D has one and only one generator. (If A is the generator of D, we write D as D_A to emphasize this fact.)

(4) D_A is a supersystem of D_B, if and only if A L-impl. B, and D_A is a subsystem of D_B, if and only if B L-impl. A.

(5)   Given a pair of deductive systems D_A and D_B there exist systems D_1 and D_2 such that:

(i) D_1 is a common supersystem of both D_A and D_B, every other supersystem of both D_A and D_B is also a supersystem of D_1, and D_1 is explicitly given as the system $D_{A\&B}$.

(ii) D_2 is a common subsystem of both D_A and D_B, every other subsystem of both D_A and D_B is also a subsystem of D_2, and D_2 is explicitly given as the system $D_{A\lor B}$.

The system D_1 of theorem (5, i) we call the *synthesis* of the systems D_A and D_B, writing the synthesis as $D_A + D_B$ (so that we have $D_A + D_B = D_{A\&B}$). The system D_2 of (5, ii) we call the *section* of the systems D_A and D_B writing it $D_A \cdot D_B$ (so that $D_A \cdot D_B = D_{A\lor B}$).

We continue the list of definitions with a concept of interest in connection with extension of deductive systems.

Def. B: Given a proper deductive system D: a system D' is *maximal*
to D if and only if
a. D' is a supersystem of D,
b. D' is not a pseudosystem,
c. there is no other deductive system D'' such that D'' satis-
fies a and b and at the same time is a supersystem of D'
while being non-identical with D'.
A given deductive system D is maximal, if and only if it
satisfies b and c above (with omission of the reference to
a in c).

Concerning the notion of maximal deductive system we give the
theorems:

(6) Every proper deductive system may be extended to a maximal
deductive system.
(7) Every proper deductive system is the (set-theoretic) intersection
of the maximal deductive systems which contains it.
(8) If D is a maximal deductive system, then for every A_i either
$A_i \varepsilon D$ or $\bar{A}_i \varepsilon D$ (and at most one of them).

Usually a set of sentences (A_i) does not satisfy the conditions of
def. A_1; therefore we introduce an operation 'Cn' which to a given
sentence set (A_i) coordinates a set $Cn((A_i))$ which is the minimal
deductive system containing (A_i), i.e. the deductive system generated
by the set (A_i).

Def. C: Given a sentence set (A_i), by the set $Cn((A_i))$ is understood
the deductive system such that
a. $(A_i)_{\mathbb{C}}Cn((A_i))$,
b. if D' is a deductive system and $(A_i)_{\mathbb{C}}D'$, then
$Cn((A_i))_{\mathbb{C}}D'$.

Some simple theorems are:

(9) If (A_i) is a deductive system, then $Cn((A_i)) = (A_i)$.
(10) Given a deductive system D with generator A, $Cn(A) = D_A$.
(11) To every set (A_i) there exists an A such that $Cn((A_i)) = Cn(A)$.

The operation 'Cn' is arithmetically described by:

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(12) a. $A \mathbin{\subset} Cn(A)$.

    b. $Cn(Cn(A)) = Cn(A)$.

    c. $Cn(AvB) = Cn(A) \cap Cn(B)$.

    d. $Cn(A) \cup Cn(B) \mathbin{\subset} Cn(A\&B)$.

    e. $Cn(A) \mathbin{\subset} Cn(B)$ if and only if $Cn(\bar{B}) \mathbin{\subset} Cn(\bar{A})$.

The connection between the set-theoretic operations 'ʊ' and '∩' and the deductive system operations '+' and '.' is as follows:

(13) a. $Cn(A) \cup Cn(B) \mathbin{\subset} Cn(A) + Cn(B)$.

    b. $Cn(A) \cap Cn(B) = Cn(A) \cdot Cn(B)$.

An illustration of the significance of the concept 'deductive system' will be given in note IV in connection with the discussion of norm systems; here we just append a remark on theorem (13). We note that the section (greatest common subsystem) of two systems coincide with the set-theoretic intersection of the sets of sentences which the systems constitute; this appears reasonable in an intuitive interpretation. But the set-theoretic union of two deductive systems is usually only a sub-system of the synthesis of the same systems, which is as it ought to be, because the conjunction of sentences from both the systems may give consequences contained neither in the one nor the other of the given systems.

With every admittable expression of a calculus of type K there may be associated a value series of length m (m differing for the various calculi, $m = 2n$ for Sk_n etc. $m = 11n$ for bF_n). Utilizing some simple combinatorial results we may obtain results of the type:

(14) The number of possible deductive systems in K is $2m$, of maximal deductive systems it is m.

If by the *strength* of a deductive system D we understand the number of f's in the value series coordinated to the generator of D, we have:

(15) The number of deductive systems of strength k $(1 \leq k \leq m)$ is $\binom{m}{k}$.

We have a natural terminology in saying that A_i is a *consequence* of the system D, if and only if $A_i \varepsilon D$, and that A_j is a *presupposition* for A_k, if and only if $A_k \varepsilon Cn(A_j)$, hence:

(16) Given D of strength k: the total number of consequences is 2^k, the number of consequences of strength j $(1 \leq j \leq k)$ is $\binom{k}{j}$. Similarily the number of subsystems is 2^k, etc. Analogous results may be obtained concerning presuppositions and supersystems (the numbers is 2^{m-k} and $\binom{m-k}{m-j}$).

The constructions of this note are effective, but it should be remarked that a similar theory can be developed with other and non-effective logical calculi as underlying systems, though then, e.g. proofs of existence no longer will in general be constructive.

Returning to a problem stated at the beginning of this note: to the initial problem of compatability of the set (A_i) we may coordinate the formal problem of investigating whether $Cn((A_i))$ is pseudo or not. But the question of relevance of the formal problem to the initial one still remains open.

IV. This note will suggest an application of the concept of deductive systems within the study of norm systems. The study of norms received little attention in the earlier period of empiricist philosophy, norm expressions being denied the status of scientific acceptability. Hence it may be of some special interest to outline a possible method of application of logical calculi within this problem field.

First some considerations on a heuristic level will be given, leading up to the definition of a concept 'normative system'. The fundamental notion is that a normative system (intuitively considered) shall give a systematization of sets of norms, laying bare their mutual deductive relationship. A norm system (being a set of sentences of some suitable structure) may contain both factual sentences and norm expressions in order to ensure broader applicability of the proposed concept. So the first problem is what type of norm expressions and what type of factual sentences are to be admitted.

As regards the structure of norm expressions, we may single out for attention sentences giving directions of conduct in relation to some set of acts and situations; hence the concepts of obligation and permission present themself as suitable candidates for determining the structure of norm expressions. Universal agreement on this point has by no means been reached, but it is believed to be of sufficient interest for applications to consider operators 'obligatory' and 'permitted'. Thus the sentence type admitted as atomic for a normative logic will be "The act . . . is obligatory (permitted) in . . . situation", with suitable symboli-

zation 'O(A, S)' and 'P(A, S)', the letters 'O' and 'P' standing for the operators 'obligatory' and 'permitted'. We assume at the outset that there are finite sets of acts and situations (or rather act-types and situation-types). This assumption will be discussed below.

Considering the possible types of factual sentences, the need of introducing modal operators such as 'necessary' and 'possible' is apparent. We may want to have as valid the rule of inference that if the act A is obligatory in S, and A implies B with necessity in S, then B shall be obligatory in S, considering it not in accordance with intuitive notions to admit as valid the rule that if A is obligatory in S and implies B in S, then B shall be obligatory in S, because the proposition that A implies B in S, with the use of material (truth functional) implication, may not be regarded as a sufficient ground for accepting B as obligatory if A is. Hence we decide to admit as atomic factual sentences for a normative calculi expressions 'N(A, S)' and 'M(A, S)', the letters 'N' and 'M' standing for the modal operators 'necessary' and 'possible'. Here it should be remarked that the modal operators may be given alternative interpretations, one in the current sense of logical necessity and possibility, another in terms of what may be called "practical necessity (possibility)" introduced in connection with derivability within a hypothetico-deductive system, i.e. an act implies another with necessity in a certain situation if the performance of the first implies the performance of the other relative to an established hypothetico-deductive system. However, the problem of interpretation is irrelevant for the formal construction to follow, but, of course, of prime importance in evaluating the fruitfulness of the construction.

Having thus introduced the types of norm expressions and factual sentences for a normative system, we may consider some possible problems to be treated.

a. First the problem of compatibility of norm sets: Is a given set of norm expressions simultaneously assertable without involving any logical falsehood?

b. The question of deducibility: Is one set of norms derivable from another set, what factual sentences must be presupposed to derive a given norm set from another, what are the consequences of a certain norm set, — and similar questions.

c. The problem of independence: Is a given set of norms independent of another, relative to a set of factual sentences?

d. Extension and systematization of norm sets: Given a set of norms, what normative systems may be found (relative to a specified set of

acts and situations) which contain the given set as consequences. Further, given a normative system, in what ways may it be extended? Is there a normative system incorporating a given one which for every pair of act-situations (of the considered underlying set) specifies if the act in this situation is obligatory (permitted) or not? May every normative system be extended to a "complete" normative system relative to an act-situation set?

Because these and similar types of problems seem to be of sufficient interest within the study of norm systems, supplying a formal apparatus should be considered a fruitful approach (i.e. satisfying a requirement of adequacy in problem field description). Granted a calculus of type K for a normative logic, we may easily introduce a concept 'normative system' in accordance with our criterion of simplicity, using the concept of deductive system given in the preceding note. The next note will outline a simple system of normative logic generated from atomic expressions 'P(A, S)' and 'M(A, S)' by use of the sentential connectives, and an associated algorithm will be supplied. Making use of this fact we introduce the definition:

Def. I: A *normative system* N is a (non-empty) set of sentences (within a calculus K_N of the above described type) satisfying:
          a. N is a deductive system.

We note that every normative system contains at least one norm expression, although the norm expression may be an L-truth. By this definition we have supplied an exact framework for treating the problems a to d suggested in the above list, i.e. granted some hypothesis of empirical semantic nature guaranteeing the relevance of the formal problems to the initial ones.

Now the results of note III can be applied to the present concept of normative system. It may be established that every normative system has one and only one generator (expressing the assumptions for that normative system). Further we see that every pair (or any (finite) set) of normative systems has a synthesis and a section. A concept of maximal normative system is obtained and we have a class of associated theorems. As regards the concept of maximal normative system we have possibilities of making a distinction. First we may consider the concept of maximality as transcribed from note III, but we may also consider a concept of maximal normative system relative to a specified set of factual sentences (i.e. the set of factual sentences asserted is left invariant by the exten-

sion). Using the second concept of maximality we need a restriction in theorem (8). If N is maximal of the second type, then for every *pure* norm expression either it or its negation is contained in N.

The numerical descriptions of note III similarly extend to normative systems as here introduced. Our constructions at the present stage are adapted to the concept of local applications, and this being so, it is believed, not trivial problems only may be considered. Suppose we had given (empirically) some set of norms. If we can transform our original problems so that a mapping of the initial problems on problems pertaining to the formal framework as here supplied, then we may consider, with the assurance of effectivity in problem solution, possibilities of systematization and the resulting systems' deductive interrelationships. We may further study the possibilities of distinct extensions of given norm sets retaining the property of consistency, we may consider synthesis and common content of classes of systems, and we may investigate the possible consequences of a given norm set. Surely, if adequacy in description is obtained, these are not entirely trivial problems.

At this point we ought to consider the soundness of the approach, giving some brief remarks on a possible charge of excessive mathematization, on claims made and especially not made in connection with the method, and on the assumption of a finite domain of acts and situations.

First we must meet a charge of excessive mathematization within a field so far removed from mathematics as to make any application of formal methods within it both irrelevant and unsound. Nowell-Smith says something to this effect in his recent book (*Ethics,* Lnd. 1954) when he asserts that the use of mathematics as a model for formal logic makes it singularly inappropriate for application in the study of norms. It is tempting to try a check-mate in two moves: Science is reasoning, reasoning is mathematics, hence science is mathematics. This is, of course, not any serious reply to a possible charge of excessive mathematization, but it contains a kernel of truth. What has been attempted is not to make mathematics out of ethics, but to explicate the modes of reasoning used, and formal techniques have proved to be of great use in this task of explication. It is not by any means claimed that what has here been briefly described is the one and only satisfying form of systematization within the field of norm studies, but it is believed that with the notions of local application and algorithmic formal methods a possible technique has been provided to handle problems within a broader field without changing the character of these problems (i.e. the formal problems are the "same" as the initial ones). Our attempt to

apply formal methods in the study of norms must not be construed too pretentiously. The investigation of deductive properties of sets of norms is by no means all that is relevant in connection with norm systems. It is merely one aspect of a larger problem field.

Secondly, we have presumed the existence of a domain of acts and situations of finite cardinality, i.e. we have supposed that at the outset a finite number of atomic acts and situations are supplied. This assumption does not seem too restrictive, as for a given application (of local nature) we may hardly consider more than a finite number of act-types and situation-types. Of course we may have an infinity of acts covered by a certain set of norm-expressions, but we assume that they may be subdivided into a finite number of act-types. Further we are not involved in any dubious considerations of acts and situations absolutely atomic. Working with the idea of local application we assume a set of acts all different and independent, i.e. each act is distinguishable from every other in the set and the performance of one act does not logically entail the performance of another. Thus an atomic act is an act not further decomposable for the specified set of acts and situations, no claim of any "absolute" atomicity need be made (similarly with the use of 'atomic situation').

It may be of interest to suggest an application of the above concept of normative system to the problem of norm conflict. A genuine norm conflict can, of course, not be solved by formal procedures; it contains an "irreducible" choice of what to do in a certain situation, but this choice may be reduced by formal procedures to a normal form. Here we will outline such a reduction of the problem of norm conflict to the adoption of what may be called *rules of preference*.

We presume the existence of a class of norm sets $n_1, n_2, \ldots, n_p$ relative to a specified set of acts and situations (and factual sentences). By the *internal consistency* of a norm set n_i we mean that the conjunction of the norm expressions within n_i is not L-false. We may assume that each set n_i is internally consistent. (If not, subdivision may be used to obtain this property).

Next we embody the norm sets n_i in normative systems $N_1, N_2, \ldots, N_p$, such that for every i, $n_i \subset N_i$ (further that each N_i contains the given set of factual sentences). Now we introduce the concept of *external consistency* of norm sets n_i; we say that the class (n_i) is externally consistent if the synthesis of the corresponding systems (N_i) is not a pseudo-normative system. If the class (n_i) is externally consistent, we may construct the synthesis N of the systems N_i, giving the complete

set of rules of conduct in relation to the specified set of acts and situations following from the assertion of the norm sets (n_i). There is no problem of norm conflict.

It may happen that the synthesis of the systems N_i is a pseudo-normative system; then we have a situation of norm conflict, i.e. incompatible rules of conduct concerning at least one pair of act-situation follow from the assertion of all sets n_i. It seems reasonable that every type of norm conflict, if norm conflict is understood as the presence of incompatible rules of conduct in relation to an act in a situation, can be described as above, provided the norm expressions can be "translated" within a normative calculus usable as an underlying system for the theory of normative systems. Thus a situation of norm conflict in the sets n_i is characterized by the fact that the synthesis of the systems N_i is pseudo.

In a given situation a norm conflict must be resolved by adopting one of mutually incompatible rules of conduct. We shall describe how this choice can be reduced to a certain normal form. If the systems N_i are externally inconsistent, we may effectively construct systems $N_1^+, N_2^+,$ $\dots, N_q^+$ such that each system N_j^+ is proper and is a synthesis of a subset of the systems N_i, and no other system N_k of the class (N_i) can be found such that $N_j^+ + N_k$ is proper, i. e. each N_j^+ is a maximal consistent synthesis relative to the set (N_i). In regard to a given pair of act-situation we thus have two classes of systems (N_k^+) and (N_1^+), each of the classes giving compatible rules of conduct in relation to the act-situation pair, the two classes giving incompatible norm expressions (and it is not excluded that one of the system classes is empty, neither that the classes are external inconsistent, i.e. the synthesis of e.g. (N_k^+) may be pseudo).

A situation of norm conflict may now be resolved by adopting *rules of preference* stating which conduct-direction rule, either the one associated with (N_k^+) or the one associated with (N_1^+) shall be adopted. Further, possibilities of norm conflict may be investigated by the above reduction procedure, and rules of preference may be stated in advance to cover situations of norm conflict in the future. In connection with a war-resistance system rules of preference may be relevant. We may have as the "supreme" norm that violence is forbidden in every situation, but there may also be admitted rules of conduct permitting violence if the situation is so and so. In those types of situations there is norm conflict, and a total normative system of war-resistance would be pseudo. But this may be escaped by the above reduction procedure, constructing

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sets of proper normative systems, and appending preference rules to resolve the possible norm conflicts.

One may object to this type of round-about construction. Why not abandon this "supreme" norm and state a suitable modification? But who wants to have a normative system asserting as a fundamental norm that non-violence is obligatory in every situation except that if the situation is so and so, this or that, etc., then violence may be permitted. Further, the above procedure may be of interest when investigating empirically given norm sets.

It should not be forgotten that the outline given above is a reduction of a problem to a certain normal form; so far formal methods apply and may be fruitful. But the problem we are considering, that of norm conflict, is not a formal problem and may not be solved by applications of the formal apparatus only, and we are left with an "irreducible" choice-situation, here with the adoption of a specific preference rule.

V. The purpose of this note is to give a simple system of normative logic applicable as a calculus K_N in the definition of normative system. It is not by any means claimed that this system is the system of normative logic, but it is rather to be conceived as an introductory system for the construction of more extensive types for use in the study of deductive interrelationship of norm sets (just as the sentential calculus is necessary as an introduction to the quantificational calculus).

The following system is obtained by a simple extension and combination of some systems of alethic and deontic modal logic as given by von Wright (*An essay in modal logic,* Amst. 1951).

First we suppose given two sets of objects, called 'acts' and 'situations' (to indicate the interpretation). Atomic acts (cf. the use of 'atomic', note IV) are denoted by letters 'a_1', 'a_2', ..., 'a_n', atomic situations by 's_1', 's_2', ..., 's_m'. We assume the domains of acts and situations to be finite for a certain application of the normative logic (to ensure constructivity in the theory of normative systems). This assumption was discussed in note IV. Compound acts and situations are constructed out of given ones by use of the connectives '&', 'v', '⊃', '∽', and '—', their rules of formal manipulation being the same as when the connectives are considered as sentential connectives (in short we assume the domains of act and situations to be finite Boolean algebras). Then we admit as atomic sentences the expression types 'M(A, S)' and 'P(A, S)' (with the interpretations indicated in note IV). The set of admittable expressions is generated by use of the sentential connectives. (A remark on

the concept of negation act: 'not to speak the truth' does not mean the same as 'to speak the not-truth'; in the interpretation we must take care not to construe the negation-act as the contrary act, but as the contradictory act, otherwise some of the L-true sentences may give implausible natural language counterparts).

The other operators are introduced by definition:

Def. A: 'N(A, S)' for '—M($\overline{\text{A}}$, S)'.
        'O(A, S)' for '—P($\overline{\text{A}}$, S)'.
        'F(A, S)' for '—P(A, S)'.

The constructed system (as pointed out by von Wright for his system of alethic and deontic modal logic) has many similarities to the system $q(F_1, \ldots, F_m)$ of note III. We introduce the definitions:

Def. B: 'M(AvB,SvT)' L-equiv. 'M(A,S)vM(A,T)vM(B,S)vM(B,T)'.
        'P(AvB,SvT)' L-equiv. 'P(A,S)vP(A,T)vP(B,S)vP(B,T)'.

To get a combined system of alethic and deontic modalities we have to introduce a defining relation between the primitive operators 'M' and 'P'. Because we are interested in investigating if a certain norm expression is derivable from another norm expression in conjunction with factual sentences, we pose as a criterion of adequacy of the system that

(1)   'O(A,S) & N(A⊃B,S) . ⊃ . O(B,S)' is L-true.

We may show (by introducing suitable truth value procedures) that of reasonable candidates for defining relations, 'O(A,S)⊃M(A,S)' is not sufficient as a presupposition for (1), whereas 'P(A,S)⊃M(A,S)' is sufficient but not necessary. If we adopt the first one we get as L-true 'O(A,S) & N(A⊃B,S) . ⊃ . P(B,S)' which may be considered acceptable as regard the interpretation, but too weak in deductive power. Hence we adopt the second candidate, but observe that the construction of a combined system is perfectly feasible using 'O(A,S)⊃M(A,S)' as a defining relation between the primitive operators.

Def. C: 'P(A,S)⊃M(A,S)' is L-true.

Now we are in position to give a truth value algorithm associated with the calculus constructed, and the procedure is strongly reminiscent of the one used in connection with the calculus $q(F_1, ., F_m)$. First we

transform in a given expression 'M(A,S)' or 'P(A,S)' both components of the operand into the principal disjunctive normal form to obtain that every admittable expression is given as a truth function of elementary expressions

(2) $M(a_1^+ \& a_2^+ \& . \& a_n^+, s_1^+ \& s_2^+ \& .. \& s_m^+)$.
        $P(a_1^+ \& a_2^+ \& . . \& a_n^+, s_1^+ \& s_2^+ \& .. \& s_m^+)$.

Then the notion of associated index set is introduced as in note II, and a rule analogous to rule D of that note is introduced to obtain elementary expressions of the form

(3) $M(P_i, Q_j)$, $P(P_i, Q_j)$ with $1 \le i \le 2^n$, $1 \le j \le 2^m$.

Then in addition to definition C we have to introduce the following defining relations (analogous to (17) of II)

Def. D: a. '$M(P_1, S) v M(P_2, S) v \ldots v M(P_{2^n}, S)$' is L-true.
         b. '$P(P_1, S) v P(P_2, S) v \ldots v P(P_{2^n}, S)$' is L-true.

It is easily seen that a of def. D is derivable from b by use of def. C. We have stated it in order to stress the parallelism between the systems of alethic and deontic modalities, and because some other relation instead of def. C may not ensure the derivability of def. D, a. Without entering into detail (the result should be obvious) we assert that by giving a rule of value series correspondence similar to rule E of II (but more complicated because of more defining relations to pose restrictions on the value series map) we obtain that to every admittable expression within the calculus corresponds one and only one value series (the mapping being one-to-one between value series and equivalence classes of admittable expressions), provided we take care of the case when the first operand component is contradictory by introducing a rule similar to the one given in connection with the calculus $q(F_1, \ldots, F_m)$ in the analogous situation.

Thus the task of giving a calculus K_N for def. I of note IV is completed.

Before giving attention to some questions of detail in connection with the above calculus, we stress the restricted level of pretention as regards the above construction. Yet we have an inadequate calculus for satisfactory problem description. We lack means to express time dependent

norms. The need of introducing (restricted?) quantifiers is obvious, although this defect may to some degree be circumscribed by use of free variables with the generality interpretation. Further, the assumption of finiteness as regards the domains of acts and situations should be weakened (to allow, e.g. an enumerable infinity) for some applications, and perhaps some other primitive domains than those of acts and situations could with advantage be introduced — there is no want of problems in connection with a normative logic.

Concerning the above calculus we remark that the (not stated) rule concerning contradictory first component of operands gives the L-truth of 'N(Av$\overline{A}$,S)' and 'O(Av$\overline{A}$,S)', which if not covered by common usage, hardly contradicts it. Further the definition D is equivalent to asserting the L-truth of 'O(A,S)⊃P(A,S)' and 'N(A,S)⊃M(A,S)', or in another form 'M(A,S) v M($\overline{A}$,S)' and 'P(A,S) v P($\overline{A}$,S)'. We also have as L-true the expression 'O(A,S)⊃O(A,S & $\overline{S}$)', i.e. granted an act is obligatory in at least one situation, it is obligatory in the contradictory (or impossible) situation. Analogous we have the L-truth of 'P(A,S)⊃P(A,Sv$\overline{S}$)', i.e. if an act is permitted in at least one situation, it is permitted in the analytic (or vacuous) situation. Concerning the L-truth of either 'O(A,S & $\overline{S}$)' or 'P(A,Sv$\overline{S}$)' the calculus gives no decision, and that may perhaps be the safest course to choose.

From the L-truth of

$$(4) \qquad O(A,S) \;\&\; O(A,T) \,.\, \supset .\, O(A,SvT),$$
$$O(A,S) \supset O(A,S\&T),$$

we may give a characterization of the set of situations in which an act is obligatory (formulas (4) say that this set of situations form an ideal in the Boolean algebra of situations). The first expression says that if an act is obligatory in both of two situations, it is obligatory in the compound situation present if at least one of the situations is present. The second formula says that if an act is obligatory in a given situation, it is obligatory in every situation contained in (i.e. whose presence L-implies the presence of) the given situation. From our assumption about the domain of situations we may establish that if an act is obligatory in at least one situation, there exists a situation (which may effectively be described) such that the act is obligatory in this situation and in every situation contained in this situation, but is obligatory in no other situation. Hence we have a complete determination of the set of situations in which an act is obligatory. As we do not have the

L-truth of 'P(A,S) & P(A,T) . ⊃ . P(A,S&T)', which seems reasonable in the intended interpretation, we do not have a similar description of the set of situations in which an act is permitted, although we have the L-truth of

(5)   P(A,S)⊃P(A,SvT),

i.e. if an act is permitted in a given situation, it is permitted in every situation which contains the given one.

We have indicated an interpretation in terms of acts and situations. This is not the only possible interpretation; for acts we may substitute attitudes and for situations groups of persons, thus creating a set of four possible interpretations of the calculus. An almost trivial extension of the calculus to elementary expressions of the type 'M(A,S,P)' or 'P(A,S,P)', i.e. the act A is possible (permitted) in the situation S for the person group P, can be obtained, increasing the adequacy in description without introducing further technical difficulties, hence retaining the requirement of simplicity.

This simple system of normative logic is described in some detail in a forthcoming paper by the present author (*Notes on Normative Logic*, Vitenskapsakademi, Oslo).

VI. Trying to apply formal logic within the soft sciences, a problem should be of immediate concern. Usually there is given an initial problem, i.e. a problem stated in the language we use, being perhaps both vague and ambiguous. Then we end up by considering a formal problem, i.e. a problem treatable within the formal framework given. By some formal manipulations a conclusion is reached, and then the fundamental question is: Is the conclusion to the formal problem an answer to the problem initially considered? Is the formal problem in some sense the "same" problem as the initial one? This is the question of relevance, and the reply to it determines to a large extent the fruitfulness of the formal approach. It is a thesis of this paper that the question of relevance is of empirical semantic content, and hence that applications of formal methods are to be accompanied by empirical semantic investigations (as regards empirical semantics reference is made to Næss: *Interpretation and Preciseness*, Oslo 1953).

To consider the situation in more detail a concept of *application field* is introduced (or rather described) in this paper. We have a formal calculus K and the language we use L; the task is to supply some connection between K and L (usually stated as the problem of symboli-

zation and desymbolization). An application field corresponding to a certain calculus is a set of sentence-schemas within the language we use, one schema corresponding to each atomic expression within the calculus. A particular application field is a set of sentences obtained from the sentence-schema by substitution. As regards the structure of the sentence-schema within the application field (AF) it suffices here to say that they are of the "same structure" (in a intuitive obvious sense, our discussion is heuristic) as the atomic expressions of the specified calculus. Further the AF shall contain words corresponding to the sentential connectives (and possibly other connectives, operators or functors). The correspondence between an AF of a K and the K shall by definition be an isomorphism, thus by giving an AF to K we have "translated" the formal expressions into sentences within the language we use. But it should be stressed that the AF at this point is regarded intrinsically, i.e. not viewed as a part of the language we use. So by giving only an AF we have not yet brought about a connection between K and L; that is first obtained by viewing AF as a part of L. The isomorphism mapping beween K and AF is introduced by definition, and is quite unquestionable, as the AF at this point has nothing to do with L. The problem arises when considering AF as a part of L (i.e. by mapping AF canonically into L, in usual terminology). Is a rule of inference in L obtained from K by AF intuitively acceptable? Does it correspond to actual usage? There is an obvious need of establishing a set of empirical semantic hypothesis to obtain relevance in application of formal methods to problems within the language we use.

Given an initial problem (i.e. a problem stated within L), as a first step in applying formal methods, a transformation of the initial problem must be given such that a problem of AF-structure results, with a suitable particular application field. The problem is still a problem within L and a suitable application field can be found if our formal apparatus satisfies a requirement of adequacy in description. Then using the inverse of the mapping of AF into L, we have a problem stated within AF (or perhaps the transformation of the initial problem in L has resulted in a problem about some set of sentences within AF, because in L usually no distinction between the object language and the meta-language is made). Hence by using the isomorphism mapping between AF and K we have transformed the initial problem into a formal one. Granted a solution of the latter we may proceed the other way, obtaining an "answer" to the initial problem as the map of the formal problem solution by AF. Is this "really" an answer to the initial

problem? This is an important question, and it involves empirical components, the answer to it depending both on the adequacy of introducing the canonic mapping of AF into L and the problem invariance in regard to the first transformation within L. The last requirement is, considering a specific initial problem, that the transformation of it within L to an AF-structure problem shall leave invariant the content of the given problem. This necessitates the establishment of some synonymity hypothesis requiring empirical procedures (in conjunction with such other methods as are found to be of use). We should remark that an initial problem may be so vague and ambiguous that it has to be transformed into a set of AF-structure problems. (At this point reference to the discussion of J. Meløe in his *Dialogue on the hypothetical character of logical analysis* (this journal vol. 1, no. 1) is particularly appropriate.)

The above discussion has been heuristic and thus preliminary, but it is beyond the scope of these notes to introduce it on a precise technical level (e.g. by introducing satisfactorily a concept 'application field'). But although these remarks have been heuristic, they should justify the thesis that investigation of empirical semantics is necessary in the application of formal logic to non-formal science, thus stressing the need of cooperation between the formalist and the empirical semanticist in the application process.

We close this note by some remarks relating the above to the considerations of note IV, concerning the study of norm systems. We gave there an answer to a possible charge of excessive mathematization delivered by the analyst within the context of natural language. We may here continue by determining the jobs of the analyst and the formalist within this problem field. The methods may seem to be in opposition. They ought to be complementary, the analyst taking care of the initial problem transformation within L, attempting to establish the sameness of problem content, the formalist attending to the manipulations within the calculus used. It should not be forgotten, however, that this discussion concerns only deductive properties of norm sets, which does not by any means cover the complete field of norm studies.

It need hardly be remarked that any possible charge of the fallacy of *unum nomen unum nominatum* is no longer of any relevance to the formalist if the above division of labour is carried through. But it should be stressed that this division requires the analyst within the context of natural language to be empirically conscious, which implies the readiness to apply empirical methods of investigation to problems still in the speculative stage.

THOUGHT AND ACTION

by

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1. In his recent article, "The Theoretical Relations of Thought and Action," Charles Perelman makes in brief compass a number of judgments which are worthy of closer scrutiny. Perelman says: "contemporary philosophy, when it examines the theoretical relations of thought and action, gives without hesitation the superiority to action, which becomes the criterion of the value of thought." This decision, he continues, "is completely opposed to the classical tradition of western metaphysics . . . since Plato and Aristotle." After presenting a sketch of this "classical tradition," which for Perelman is essentially "theological" in its vision of an eternal order that provides the definitions of truth and freedom, he offers some suggestions toward a theory of practical reason in accord with contemporary emphases and interests.[1]

As Perelman's title well emphasizes, the problem of the relationship between thought and action is theoretical. Disagreement with contemporary approaches to this problem in no way implies criticism of the practical consequences of empirical techniques, but of the way in which these results are explained. It may be that corrections are desirable in empiricist epistemology. In this paper, two suggestions will be made. The first will be put most concisely: namely, that it is theoretically unintelligible to say that action is the criterion for the value of thought. The second will require considerably more expansion: Perelman's picture of the classical tradition, by no means peculiar to him but representative of much influential contemporary opinion, is a misleading blend of Greek and Christian rationalist elements. In this mixture, the Christian element predominates: it is consequently not seen that Greek rationalism

has features which should be of great interest to recent empiricism on the subject of thought and action. My own view is that empiricism, despite whatever it may *say,* cannot really wish to *mean* that action is the criterion for the value of thought. If I say that empiricism is itself a version of rationalism, I do not pretend that it is identical with Greek rationalism. But, having once distinguished the Greek from the Christian version, it seems possible to restate the former in such a way as to make it deserving of more serious attention by imaginative empiricists.

2. If action is the criterion for the value of thought, how are we to formulate the relationship which must necessarily exist between the two? Either thought and action are at bottom the same, or, if different, their connection must be such that action is intelligible apart from thought. Let us first assume that they are different. If it is thought which makes action intelligible, then either thought furnishes by that token the value of action, or the intelligibility and value of action have nothing to do with each other. In the former case, the view that action is the criterion for the value of thought is directly refuted. In the latter, two new alternatives arise. Given, that is, that the *value* of action does not arise from the *intelligibility* of action, then the value of action is itself either intelligible or unintelligible. If the value of action is unintelligible but still the criterion for the value of thought, then the unintelligible has jurisdiction over the intelligible. And so, if value is unintelligible, there are no intelligible criteria according to which one theory may be preferred to another. It is important to notice that no question of "moral" value is being raised here. The consequence is entirely general: *theory becomes theoretically unintelligible.* If the empiricist insists that theories are in fact discriminable rationally on the basis of their concrete results, he denies this consequence. It seems fair to say that empiricists and rationalists alike would agree that the value of action is at least intelligible. But what is the source of this intelligibility? Either thought or action. But if thought, then again we arrive at the result that action cannot be the criterion for the value of thought. Thus, under our initial assumption that thought makes action intelligible, the intelligibility of the *value* of action must be furnished by action itself. But is it even intelligible to speak of the intelligibility of the value of action as distinct from the intelligibility of action tself? In the present context, the "value" of action can only mean its power to act as a criterion by which we construct and discriminate among theories. When we say that action is intelligible, in this con-

text surely we mean precisely that the *value* of action relative to thought is intelligible. I do not see how anyone, empiricist or otherwise, could deny this, or wish to deny it. But for the sake of argument, let us presume it to be denied. Then some such consequence as this will follow: there is a quality "value," which is distinct from both thought and action, intelligible through some capacity X, not itself thought or action (not action because action is intelligible through thought by hypothesis), but which is so related to action as to provide the means whereby thoughts may be intelligibly discriminated. Apart from the fact that no empiricist could legitimately speak of a "capacity X" without falling into metaphysics, "capacity X" as distinct from thought, the source of intelligibility, is itself the unintelligible arbiter of intelligibility, through the medium of action. Either that, or it gains its intelligibility from thought, and so consequently does the intelligibility of value, with the result once more that action cannot be the criterion for the value of thought. We are at last left with the one possibility that thought and action are, if different, so connected that action is intelligible apart from thought. What then is the source of the intelligibility of action? I suggest that there is no way even to speak of the intelligibility of action without referring it to thought, or to some capacity X. We have already seen what results from introducing an "X capacity." Barring an "X," actions are intelligible only when we think about them. To look to action for results in terms of which thoughts (theories) are accepted or rejected is *not* to make action the criterion for the value of thought. The error lies not in submitting to experience, but in our analysis of what takes place when we submit to experience.

3. Can these difficulties be eluded by the identification of thought and action? At first it might seem so, and it is a course which most if not all empiricists *do* seem to take. It can be shown, I believe, that they are led to the identification of thought and action *even when,* in deference to David Hume, they explicitly state that the two have nothing to do with each other. Let us briefly consider this identification, both as a concrete example of some aspects of the preceding article, and in order to see that, from such an identification, the situation we are discussing is in no way changed.

According to Hume, thinking and acting are radically distinct, and grounds for acting cannot be deduced from reason:

"Reason is the discovery of truth or falsehood. Truth or falsehood consists in an agreement or disagreement either to the *real* relations of ideas, or to *real* existence and matter of fact. Whatever, therefore, is not susceptible of this agreement or disagreement, is incapable of being true or false, and can never be an object of our reason. Now 'tis evident our passions, volitions, and actions, are not susceptible of this agreement or disagreement; being original facts and realities, complete in themselves, and implying no reference to other passions, volitions and actions. 'Tis impossible, therefore, they can be pronounced either true or false, and be either contrary or conformable to reason."[2]

An indirect consequence of this argument, Hume states, is that reason cannot be the source of moral good or evil. As was noted previously, the question of the cognitive status of moral judgments is subsidiary to or narrower than the fundamental question of the intelligibility of all forms of action (and of inclinations toward action). Thus, it is not fruitful to proceed, as have many contemporary writers, by restricting oneself to an analysis of moral statements. The relationship between theory and practice, as Hume saw, is not itself a problem of moral philosophy: moral philosophy rather depends upon how we conceive of the relationship between theory and practice. On this point, I believe, Hume is within the "rationalist" tradition. But his answer to the question is the reverse of the rationalist answer: "Reason is and ought only to be the slave of the passions, and can never pretend to any other office than to serve and obey them."[3]

Action arises in response to passion, which is in turn stimulated by the prospect of pleasure or pain. (We may observe parenthetically that this prospect is either simply or derivatively "mental.") By making reason the slave of passion, Hume in effect conceives of reason as itself an activity: in the very distinction between thought and action, the distinction breaks down. This is so despite the possible counter-claim that there is a distinction between the "truth" of a proposition and the act of thinking or constructing a proposition. To begin with there is no *reason* why we should perform the activity of reasoning: reason cannot justify itself. But neither can reason give *rational criteria* in terms of which the predicate "true" is both intelligible and by definition safe from the meaninglessness of activity. The significance of determining which propositions are "true" reduces to the degree of pleasure which the pursuit

of truth arouses in us. Again I would emphasize that by "significance" is not meant anything like "transcendental purpose," but the possibility of consistent theoretical intelligibility. The truth which we pursue is in effect a crystallization of passion. Since pleasure and pain are beyond the pale of reason, it is not just the case that theory is subordinated to, or identified with, practice: *practice is theoretically meaningless.* Therefore, the roots of rational thought are outside the pale of reason. Pleasure makes no theoretical sense: to say that pleasure justifies itself is to give up theory altogether. Pleasure and pain are not just brute facts: they are *the* brute facts. To be consistent, then, any given theory, or theorizing as such, should be abandoned upon the dictates of brute pain. Little wonder that Hume gave up philosophy for history!

Since passion regulates the activity of reason, it will also determine our conception of truth. In the last analysis, truth is unreasonable. It possesses no significant characteristic beyond its truthfulness, which is itself insignificant. True propositions are no more reasonable than the products of any other activity, and the distinction between reasoning and the decision to act breaks down. If "true" is a predicate attachable only to products of an activity, and if activity itself is neither true nor false, but rationally insignificant, then the status of truth is destroyed or, at the least, seriously compromised. This argument is not merely a restatement of Hume's admission that what we think to be true could be an illusion caused by passion. Assuming that true propositions are indeed "true," there is no way *in principle* to arrive at a purely theoretical explanation of what it means to be "true" because there is no way to distinguish between thought and action. Hume's own attempt to distinguish them is at bottom a rejection of the possibility of making such a distinction.

4. Empiricism, then, either explicitly states that thought is the same as action, or is forced to admit that they are the same; but in any case, it is forced to reconsider the "rationalist" claim that thought provides the criterion for the value of action, and not vice versa. The identification of thought and action settles nothing, if it is even intelligible: we would still be required to account for the relationship between types of action which are empirically verifiable as distinct and which no sane terminological apparatus would attempt to identify. For assume that thinking is a species of action. Theoretical "doing" must still determine which configurations of non-theoretical "doing" are *to be done*. The distinction between thought and action is one of immediate experience; it

cannot therefore be removed by an appeal to immediate experience. Any subsequent analysis of immediate experience which tries to deny this distinction soon leads to unintelligibility if it makes non-theoretical doing superior to theoretical doing. But if theoretical doing is superior to non-theoretical doing, then the situation which arises is identical with that of classical rationalism. We cannot even begin to philosophize without making a distinction, in some form or another, between thought and action; nor can thought be entirely unrelated to action. The concepts by which we analyze the structure of practice do their work only because there is, within our experience and before any analysis of it, a connection and distinction between thought and action. Logical and linguistic tools, to be so utilized, must be theoretico-practical in advance. There is, then, something wrong with every version of Humean epistemology. Practical principles mut be theoretically intelligible, and this is not possible if theoretical intelligibility is a matter of practice. This is not to deny the function of observation, but to ask for a review of the reasons why we are able to observe at all.

5. In the article which has already been cited, Charles Perelman contrasts "the classical tradition of western metaphysics" with marxism, pragmatism, existentialism, and scientism. The classical tradition is in essence theological rationalism, which Perelman distinguishes from the position of "atheistic rationalism." The latter is in turn based upon "exact science." By atheistic rationalism, then, Perelman appears to mean something like a blending of pragmatism and scientism. In passing, this would seem to imply that empiricism is a form of rationalism. We have now to show that, given Perelman's characterization of western metaphysics, Greek rationalism is as anti-metaphysical as the strictest empiricist. The main points of Perelman's sketch of "the classical tradition" are these: "For the classical tradition, true knowledge and free action consist in conformity with an order existing prior to all human action" This prior order is eternal and immutable, the object of true propositions and the paradigm for virtuous action. "The free action of the wise man will follow the objective rules of morality and natural law" which are deduced from the eternal and immutable order. The architect of this order is God: "From St. Augustine to Leibniz, the Christian tradition of philosophy, including St. Thomas, Duns Scotus and Descartes, finds in God a guarantee to human thought. Since the divine intelligence knows in advance the solution of all problems, and makes these known to us by natural and supernatural enlightenment, Christian optimism gives confidence

to the philosopher. The Christian rationalist knows that all problems have their solution within eternity, and to discover it he only needs to exercise skilfully his natural faculties."

With every allowance for the brevity of Perelman's account, one is forced to wonder at the nature of its judgments. Granting that Descartes and Leibnitz speak of God, it is strange to see them bracketed with the Church fathers. Did either Augustine or Descartes believe it possible to discover the solution to "all problems" by the use of man's natural faculties? Was St. Augustine's optimism of the same "rationalist" character as that of St. Thomas? But all this by the way: our present concern is with the manner in which Perelman conceives of the "classical tradition" as a unified line from Plato and Aristotle through the Christian rationalists. At least one of his reasons for so doing emerges in his concluding paragraph: "To define truth and freedom only as conforming to an unchanging and perfect order means to support implicitly a theological vision which is that of classical metaphysics." For Perelman, then, the traditional formulation of the relations between thought and action is unambiguous, and essentially the same in Greek philosophy as in Christian theology. This radical oversimplification is both frequently encountered and entirely misleading to any serious effort to understand either the classical tradition itself, or its relationship to the tradition which Perelman, like most contemporary thinkers, himself favors.[4]

The spirit of Perelman's summary (if not its details) is perhaps most compatible with orthodox Thomism: more specifically, with the teaching of Thomas on the relation between theory and practice. In briefest compass, it is true that Thomas, in the *Treatise on Law,* constructs a hierarchy of the modes of practical precept and law, within which positive laws are grounded in natural law (the product of speculation or philosophical theory), and natural law is in turn grounded in divine law (the product of revelation.[5] Thomas solves the problem of the relation between theory and practice by appeal to a theological vision, from which is articulated the rationally knowable, eternal, and immutable structure of the universe. Human nature is defined in terms of man's position within this structure, and so man deduces the principles of practice. But Thomas' solution is considerably different from the approaches taken by Plato and Aristotle, and this is true despite Thomas' attempts to make Aristotle compatible with his own interpretation of Christianity.

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6. In the first place, for Thomas, as a Christian, the practice of virtue was an intrinsically higher human perfection than rational speculation. Since it is practice rather than theory which wins man's salvation — for salvation is accessible, not just to the philosophers, but to everyone — it is necessary to subordinate theory to practice; and this subordination is of course justified by the eternal order of God's creation. If we avoid theological terminology, it would not be unfair to say that, despite Thomas' "rationalism," the consequences of his stand on thought and action are closer to the views of contemporary empiricists and existentialists than to the views of Aristotle. Philosophy is both justified by and in turn serves to support the prior theological vision. Thus the virtuous man, even if he be a philosophical ignoramus, exemplifies the height of human perfection.[6] This is certainly not Aristotle's position. Even in the *Nichomachean Ethics,* where Aristotle makes his fullest defense of moral virtue and its importance for *eudaimonia,* it is clear that he regards moral perfection as secondary to theoretical exellence: man's highest happiness lies in the excellence of his highest faculty: *theoretiké.* He explicitly states of the theoretical life *(ho kata ton noun bios)* that it is "highest, most pleasant, and most blessed" *(kratiston, hédiston, eudaimonestatos).* The moral life *(ho kata tén allén aretén)* is second to the theoretical life because the moral virtues are purely human *(anthrōpikai),* whereas theoretical excellence is divine *(theion).*[7] Ethical virtue *(éthiké)* is not taught in the way that theory is, but proceeds from habit *(eks ethous). And it does not exist by nature (eks hou kai délon hoti oudemia tōn éthikōn aretōn phusei hémin egginetai).* What is natural is the capacity to become virtuous (or vicious) by habituation. To say nothing more than this, it is impossible to infer that there is an unambiguous relationship between our theoretical understanding of nature and our practical behavior; and so the sense in which the order of Being serves as a kind of paradigm for virtuous behavior, or in which reason could be said to *compel* us to behave virtuously, is entirely ambiguous.

Thus it is easy to see that the Thomistic deduction of the principles of justice from natural law represents an oversimplification of Aristotle's teaching. This oversimplification has its ultimate cause in the certitude of divine revelation, which forces Thomas to construct a static model of the hierarchically articulated universe. If it is "rationalism" to deduce the natures of things and the codes for human behavior from such a theoretical model, then Aristotle is no "rationalist." In Thomist "rationalism," there is no theoretical problem concerning the relation between

theory and practice because theory is for the sake of, subsidiary to, and certified by, a practical code which is guaranteed by trans-theoretical revelation. The very term "natural law," which symbolizes Thomas' resolution of the tension between theory and practice, is alien to Aristotle, as it is to all of classical Greek philosophy.[9] The conception of a "natural law" embodies the doctrine of a deductive and absolute relation between the order of Being or nature and the principles of justice. Aristotle, however, leaves this relationship in doubt. Even the passage of the *Ethics* which purports to distinguish between natural and conventional justice does nothing to remove the ambiguity. Some men believe, Aristotle observes, that justice is entirely mutable (and so not grounded in nature). "This, however, is not true in this unqualified way, but is true in a sense; or rather, with the gods it is perhaps not true at all, while with us there is something that is just even by nature, yet all of it is changeable . . ."[10] In Thomas' version of Aristotelianism, this passage is interpreted to mean that the principles of justice are immutable; it is their mode of application which varies. Averroes, on the other hand, states that the principles themselves are all changeable. Neither interpretation does justice to the ambiguity which is apparent in Aristotle's own words.[11]

7. The tendency to interpret Plato and Aristotle as ethical absolutists seems due in large measure to the Christian tradition through which they are normally read. This is not to say that the actual Greek view on thought and action is identical with the view to which Perelman subscribes. But the differences cannot be estimated if we fail to observe the tensions in the Greek view: these tensions disappear in the portrait which Perelman (and many others) supplies.

According to Perelman,

> "Atheistic rationalism must reduce the scope of its assertions to the single field to which exact science appears to provide a key, declaring the normative field, the rules of which should govern our action, as being beyond reason. Thought, capable of knowing the real, becomes incapable of justifying human behavior rationally. One has to give up a science of ends, for these are alien to the real . . ."

Although it seems from the concluding section of his article that Perelman's own position is more sophisticated than strict atheistic rationalism as it is here succinctly described, he clearly intends to contrast atheistic rationalism favorably with traditional, "theological" rationalism.

Atheistic rationalism is in effect the philosophical viewpoint of modern empirical science: a retention of some aspects of Hume's thought with a mitigation of its ultimate sceptical consequences. Perelman states, and implies where he does not state, that, by judging theory to be superior to practice, traditional rationalism commits itself to the simple view that practice is unambiguously legislated by theory. We have had reason to see that this is not true. It was observed above that there is a sense in which Thomas comes closer to empiricism and existentialism than to Aristotle. This observation should now be complemented by the following one: those "rationalists" who follow Aristotle more faithfully than does Thomas are closer to one important aspect of the contemporary viewpoint than he in the sense that they recognize a disjunction between the scope of theory and the rules of practice. They differ from the contemporary viewpoint, however, (as it is usually expressed) in regarding this disjunction as partially bridgeable by the use of reason. Whether or not the "ought" follows "deductively" from the "is," the relationship between the "ought" and the "is" is a matter for rational deliberation. And their conception of the disjunction between theory and practice is to this extent clearer than the contemporary view, in that it avoids the contradiction of both identifying and entirely distinguishing thought and action.

One must speak of various traditions of rationalism. Not only does the theological tradition differ from, say, the Greek tradition, but there are serious differences within the theological tradition itself. We have already alluded to the controversy between Averroes and Thomas. It will be useful now to consider the contrast between Thomas' Christianized Aristotelianism and the statement of Maimonides regarding theory and practice. From the statement of Maimonides, a professed Jew as well as an Aristotelian, we may notice a dimension in which Greek rationalism and contemporary empiricism move on parallel paths:

> "The general object of the law is twofold: the well-being of the soul, and the well-being of the body . . . Of these two objects, the one, the well-being of the soul, or the communication of correct opinions, comes undoubtedly first in rank, but the other, the well-being of the body, the government of the state, and the establishment of the best possible relations among men, is anterior in nature and time." (The perfection of the body) "is only possible in society, since man, as is well known, is by nature social. The second

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perfection of man consists in his becoming an actually intelligent being; i. e. he knows about the things in existence all that a person perfectly developed is capable of knowing. This second perfection certainly does not include any action or good conduct, but only knowledge, which is arrived at by speculation, or established by research."[12]

Maimonides goes on to state that the second perfection is clearly superior to the first, or political, perfection, which is said to be necessary in order that the body may be free from material distractions; these distractions, when present, interfere with the process of reasoning. The second, or intellectual perfection, "is alone the source of eternal life."

Such a distinction between theory and practice is reminiscent of that aspect of empiricism which denies the "reality" of ends, and so excludes the normative from the proper subject-matter of scientific reasoning. To say that practical principles are relative to the well-being of the body, or are human rather than divine, is to imply that, from a strictly theoretical viewpoint, the cognitive status of practical statements is suspect. As David Hume observed, "the ancient philosophers, though they often affirm, that virtue is nothing but conformity to reason, yet, in general, seem to consider morals as deriving their existence from taste and sentiment."[13] This is not the same, however, as to assert that practical statements have *no* cognitive status. Light is cast upon the classical attitude toward practical statements by the very argument which Hume employs to demonstrate the non-rational character of moral judgments. Hume's argument is confused by his habit of using the term "nature" in different senses, and by not always distinguishing which sense of the term is appropriate in a given context. But it is clear enough that, for him, moral judgments, and conceptions of justice and obligation, are not derivable from nature in the classical sense (since man is by nature selfish), nor from rational speculation (because they are not statements of eidetic relations or of matters of fact). On the other hand, moral judgments arise from impressions of pleasure and pain (themselves objects of cognition); conceptions of justice and obligation, although "artificial" in the sense of manmade, are not merely arbitrary: they are "natural" in being common to our species. Hume believed in the general invariability of human nature, and regarded man as *by nature* a political animal. It is advantageous for man to live in society, and, given man's natural selfishness and the limited amount of property, the need for, or utility

of, justice and obligation is itself natural. This natural need is satisfied by nature through the instrumentality of "the judgment and understanding." Society is absolutely necessary; laws governing justice, property and promises are necessary for society. These "natural laws," as Hume calls them, are indeed "entirely artificial, and of human invention" in that they are not themselves statements corresponding to the relations or connections existing among ideas. (From another viewpoint, of course, *all* statements would have to be regarded as artificial in this sense.) But they are nevertheless *products* of reason: they represent the restrictions which reason places upon human passions, and these restrictions are justifiable by the character of man's nature. Thus, the fact that the content of practical judgments is not factual, in no way alters the rationally comprehensible *fact* of their necessity, or even of their general character, corresponding to the invariable character of human nature. Furthermore, Hume's continual insistence upon the "artificiality" of these (also natural) practical judgments stems largely from his desire to refute the positions advocating innate moral truths, revealed codes, and eternally valid deductions from the immutable order of the universe. None of these positions represents an accurate picture of Greek rationalism. Apart from his failure to achieve clarity and consistency in the use of terms, Hume's account is marred by his oversimplification of the rationalist doctrines he opposes, an oversimplification which is similar to that of Perelman. But Hume's *own* doctrine on this particular issue, when it is reconstructed from his rambling sequence of reflections, can be reconciled (if not identified) with a sympathetic interpretation of Greek rationalism.[14] Indeed, in some ways Hume goes beyond Aristotle, as when he says that the rules of justice are "steadfast and immutable; at least as immutable as human nature."[15] If we ignore these "Thomist" elements in Hume, however, there is a striking similarity between his doctrine and the passages quoted from Aristotle and Maimonides. Morality is for the sake of the body (passions); it is necessary because of human infirmity. Practical judgments are not of eidetic relations or facts of existence; i. e. they do not speak of reality (the noetic or divine) but only of the merely human. But these practical judgments are constructed by the reason for the sake of man's own good: they are rationally designed regulations for practical conduct. Obedience to these regulations will ensure the welfare of the body, the continuance of corporeal (human) happiness, and consequently will maintain those practical conditions which are necessary for an unimpeded attention to theoretical reflection.

8. Of course, the resemblance is not complete, but it is suggestive in its own way. One may conjecture that, for the Greek rationalists, the cognitive status of practical statements lies, not in their relationship to an objective order of Being, but in their usefulness to the commitment to philosophy. This commitment to philosophy is no more empirically defensible for a Plato or an Aristotle than is the empiricist's commitment to experimental science. When Plato speaks of the "divine madness" which must leap up in one's soul like a spark if philosophy is to take place, he is, I believe, making no theological assertion but indicating in his own language that the commitment to philosophy is characterized by an inner necessity which must always be prior to any subsequent effort to justify it. This commitment to philosophy leads in turn to the positing of hypotheses, on the basis of which our theoretical *and* practical behavior is determined. Investigations of human nature, the relation between theory and practice, or the order of Being not only do not certify the philosophic commitment; they cannot take place except on the basis of this commitment and the hypotheses which it generates. The theoretical life is the highest life only for the theoretical man; depending upon the strength of his philosophical commitment and his theoretical gifts are the range of possibilities (or alternative hypotheses) open to his inspection, but this range of possibilities can be encountered only after one is committed to search for them. Nor could they, when once encountered, justify the primacy of theory for any but the theoretical: the simply practical man has neither the desire nor the capacity for such investigations. But further: the function of thought as the criterion for the value of action is defensible only on the basis of theoretical distinctions which in turn emerge from the content of one's philosophical commitment. The practice of philosophy is circular insofar as the "order" of the universe is conceived on the basis of one's commitment. This is not the same as to say that man "makes" the order, but rather that he discovers or understands it from the perspective in which he conducts his inquiries. The last decision as to which perspective is "the best" is not open to demonstration in the formal sense of that term. Philosophical relativism can never be expelled by rational demonstrations because, in the last analysis, demonstrations rest upon perspectives. The choice among perspectives must be made on other grounds, and this issue lies beyond our present concerns.

That knowledge or life is not *theoretically* futile can be inferred only from a theory (or revelation) which gives significance to knowledge or

life. Such a theory is in no way inferrible from a "disinterested" or "objective" inspection of experience. But no more could one infer from such a "disinterested" inspection that knowledge or life is futile. A "disinterested" inspection yields only a meaningless chaos of phenomena. To speak of the significance of acts or knowledge is already to move toward philosophy. *All* philosophies are by their nature theoretical (and "interested"), and from *all* theoretical viewpoints, practice is subservient to theory. The practical utility of theory does not alter this subservience, for "utility" is at bottom a theoretical concept. For this reason, utility cannot serve as the "justification" for theory. (The desire or instinct for existence is obviously not a theoretical proof that the instinct is justifiable. Merely to insist that the instinct is its own justification is to abandon theory, and so too philosophy.) Regulations governing practical behavior are by definition of practical advantage, but it is not in this that their theoretical significance lies. This theoretical significance depends upon the primacy of theory. If theory is primary, then human activity must be so arranged (if only in theory) as to preserve the possibility of theorizing. Practical advantage is then so defined as to be compatible with this preservation. The concrete forms that these definitions will take are obviously dependent upon the specific content of one's commitment (and of its ensuing hypotheses). Whatever their forms, these practical principles will derive their cognitive status, not from any "disinterested" inspection of eidetic relations of facts, but from the theoretical structure within and because of which, we have ideas, define relations, or identify facts. So long as we are under the impression that our store of "ideas" or "facts" is disinterestedly objective, and constitutes the dimension within which practical principles must be shown either to have or not to have cognitive status, we will never be able to see any connection between theory and practice. But if we pursue the hint in Hume regarding the role of reason in constructing practical principles, we are able to say that Hume's separate conception of a "disinterested" reason — one which is unable to authorize lines of conduct — is theoretically unsound. *Not even Hume consistently defends this position.* He, too makes reason the author of practical principles. The conception of a "disinterested" reason is both naive and absurd, since it rests upon the possibility of knowing what *is* without any theoretical directions for the recognition of what *is.* Any effective reason, whether of the scientist, existentialist, or rationalist, is effective precisely because of theoretical directions which are determined by philosophical commitment.

One last word to this long conjecture: to know that theory is superior to practice is not to know the concrete decisions or judgments which are theoretically sound, nor the practices which are compatible with sound theory. It is not the cognitive status of practical principles which is the problem for Greek or any rationalism, but rather the having and validating of cognitions.

9. There is a last approach to the Greek formulation of the relationship between theory and practice which may conveniently be studied in the Socratic doctrine that socio-political phenomena are man's gateway into the speculation on Being. This doctrine is usually presented in such a way as to support the thesis that, for Socrates (or Plato),[16] practical truths are deduced from our absolute knowledge of eternal Being. But such a thesis is in no way entailed by the doctrine as it emerges from Plato's dialogues. The thesis rest upon the assumption that Socrates claims to possess absolute knowledge about Being, but this assumption need only be stated to show us its falseness. Socrates' continual professions of ignorance are not just ironical or caused by modesty.[17] The *Theaetetus,* in which Socrates engages in his most extensive epistemological reflection, concludes with the admission that we do not yet know what knowledge is.[18] This admission is consistent with the doctrine of the Ideas. The Ideas are intuited only in the sense that, as the principle of intelligibility, they are the condition of knowledge and perception. Even if it is correct to say that we begin with an intuition of an Idea, it is not the case that we begin from knowledge of an Idea. The necessity of the Ideas is inferred from the structure of appearance by dialectic[19], but man is unable to acquire an exact understanding of the Ideas: he is himself restricted to viewing them through the veil of appearance, as is consonant with his own mediate existence.[20] Since our knowledge of the Ideas is always partial, and partly based upon intuitions which cannot themselves be adequately expressed in rational demonstrations *(logoi)*, it follows that this partial knowledge is tentative, open to continuous correction, permanently and intrinsically incomplete.[21] We know that there *are* Ideas, but this does not give us knowledge of *what* they are.

The doctrine that there is a connection between political (i. e. practical) things and the order of Being is not to be understood as a static or dogmatic conception of man, justice or the universe, but rather as a suggestion concer..ing the most fruitful procedure to follow in our pur-

suit of theoretical wisdom. Once again, the fact that such a procedure will have useful practical consequences is, from the theoretical standpoint, of secondary importance. This is indicated by Socrates when he tells us that philosophy is a preparation for death, or that the philosopher is a man who does not recognize his own neighbor.[22] It is implicit in the statement that philosophers will become kings only through compulsion or necessity[23]; that is, only because the survival of philosophy, and not merely the well-being of one's fellow-citizens, demands that philosophers participate in political affairs. The Socratic conception of the secondary importance of practice is in no sense analogous to the Epicurean withdrawal from the world. It regards practice from a higher viewpoint; its importance is not diminished but intensified by the philosopher's recognition of his still more important theoretical venture. It is this theoretical venture which *gives* to practice its importance.

According to Socrates, the city is an image of the soul writ large. We find in the different functions exercised within the city, a magnified and articulated representation of the functions of the soul. The soul is itself incomplete in two senses: its practical possibilities can be achieved only within society, and it is only within society that the possibility of philosophy arises.[24] It is only through philosophy that the soul comes to understand its full range of possibilities, itself as the part of Being which is an image of Being.[25] The relation between the individual soul and society is dynamic rather than static; Socratic political conservatism is due, not to absolute knowledge of Being, but to the desire to preserve a form of society which is most compatible with the pursuit of philosophy.[26] The soul is finite, and finitude is inseparable from limitation. The finite individual would be unintelligible except within society. But society must retain the finite nature of the individual; it, too, is intelligible because of limitations. The concept of limit takes its practical or political form in *moderation*, just as the incomplete character of theoretical knowledge has its practical counterpart, not in absolute practical principles, but in *prudence*.[27] When moderation is abandoned, the limits of the city dissolve, and the city begins its historical expansion toward empire.[28] In the process of this dissolution, it seems as if the limits of man's finitude are also released; he seems to become "free" to realize or discover himself, whether as a transpolitical immortal, a citizen of the empire, or an apolitical individual. From the Socratic standpoint, this "freedom" is illusory. Removal of the limits leads to the false implication that man is infinite.[29] In pursuit of an illusory infinitude, man's nature becomes distorted or cor-

rupted; the limitless, immoderate political context continues to function as a mirror of man's soul, but now the reflection is warped and almost unintelligible. In such a pursuit of self-realization, man has denied himself and fallen into a Protagorean chaos.

The justification for conservatism is theoretical; and this is not altered but reinforced by the fact that the theory conceives of man's practical advantage as best fulfilled within a conservative structure. This is not the place to decide whether the theory is sound or erroneous. We are rather interested in its formulation of the relations between thought and action, and the significance of these relations for contemporary philosophy. In the first place, contemporary philosophers should be certain that, when criticizing Greek rationalism, they are jousting neither with ghosts nor with their own reflection. No accurate contrast between contemporary and ancient views could emerge unless the ancient views are taken seriously. And when they are taken seriously, it can no longer be maintained that contemporary thought is radically opposed to ancient rationalism. Theories are not simple units; they have a structural development and an inner order of generality. Too frequently, the debate between classical and contemporary socio-political philosophy takes place on the level of a comparison of "values," the open or closed society, or the evils of slavery, when it is not based upon radical misconceptions. At this level, there are an infinity of contrasts, but the theoretical meaning of these contrasts is not apparent. The theoretical meaning tends to emerge only when one asks for the basis of the relations, in either view, between thought and action. This basis will never be found so long as we search for it only in analyses of the cognitive status of practical judgement, analyses which themselves already presuppose a decision concerning those relations. The so-called "disinterestedness" of scientific reasoning is itself an incompletely analyzed theoretical commitment. It leads to the strange consequence that action is paramount, that thought is in fact itself action, but that thought has no relationship to action. Disinterestedness can only mean a lack of interest: but reason *is* interested, and it is interested in *itself*. As I suggested in my references to Hume, there is at this level a marked similarity between Greek and contemporary rationalism. This similarity is not a historical accident, but reflects the nature of reason. It is, however, a highly abstract dimension of the nature of reason. The range of differences among philosophical theories emerges from the *results* of reason's interest in itself. To conclude: when we subordinate theory to practice, we do so either through an abandonment of theory, or on the basis of a prior theoretical commitment. When we

reject static or absolutist views of the deducibility of practical principles from knowledge of the eternal order, we are not rejecting Greek rationalism but in fact taking a parallel path. Who can say without walking along both paths which one leads to a better world?

NOTES

1 *Inquiry* (Summer: 1958), pp. 130–6.

2 *A Treatise of Human Nature* (Oxford: 1955), p. 458.

3 *Ibid.*, p. 458. The presence of "ought" in this sentence is interesting; it implies a moral judgment besides the strictly epistemological consequence which Hume thinks he has established.

4 A similar oversimplification, though moved by reasons opposite to Perelman's, is to be found in Eric Voegelin's recent volumes on Greek civilization. See my article, "Order and History," in *The Review of Metaphysics* (December: 1958).

5 *Summa Theologica*, II. 1, questions 90–108.

6 For a full discussion of this issue, see *Thomism and Aristotelianism* (Chicago: 1952), Harry Jaffa.

7 *Ethics*, X. vii–viii.

8 *Ibid.*, II. i.

9 The phrase *nomos phuseōs* occurs but once in pre-hellenistic Greek literature, in Plato's *Gorgias*, 483 D–E, where it is used by Callicles in a sense exactly the reverse of Thomas': namely, that nature is on the side of the strong. This was in essence the doctrine of the sophists, and Plato's attribution of the term to Callicles, the spokesman for the sophists (compare the speeches of Thrasymachus in Bk. I of the *Republic*), indicates his rejection of it as unphilosophical. To say that nature is on the side of the strong is to commit oneself to a rule that is both inflexible and irrational. The later Stoic doctrine of natural law is not relevant here. It represents already a falling away from the subtlety of Greek thought through Aristotle, and toward an inflexibility which has often and mistakenly been read back into the great Hellenic thinkers.

10 *Ethics*, V. vii: *kaitoi para ge tois theois isōs oudamos; par'hēmin d'esti men ti kai phusei, kinēton mentoi pan.*

11 For an interesting attempt to explain the ambiguity, see Leo Strauss' *Natural Right and History* (Chicago: 1953), p. 162. Strauss attributes to Plato and Aristotle the following view: "There is a universally valid hierarchy of ends, but there are no universally valid rules of action." Prudence must decide what is to be done in each concrete situation.

12 *Guide For The Perplexed* (Routledge: 1947), III. 27; pp. 312–3. See also I. 2, pp. 14–16, in which Maimonides makes explicit the inferior character of moral judgments to rational knowledge by means of an interpretation of the story of the Garden of Eden.

13 *An Inquiry Concerning The Principles Of Morals* (Open Court: 1947), p. 2.

14 *Treatise (op. cit.)*, IV 458, 468, 471, 489, 496, 521, 526, 619–20.

15 *Ibid.*, p. 620. For an account of Hume's desire to construct a political science, see *Hume's Intentions* (Cambridge: 1952) by J. A. Passmore.

16 I am here avoiding the question of how to distinguish between the views of Socrates and those of Plato because it would take us too far astray into a discussion of method-

ological problems. In my view, the question is here irrelevant insofar as, with respect to "absolute knowledge" no simple distinction could be drawn between the opinions of the two men. Strictly speaking, Plato never said anything in his own name except for his *Epistles*, in which he makes explicit this reticence by disavowing all written statements which purport to contain his teaching, *including his own dialogues.* I do not suggest that, for an ultimate understanding of the dialogues, one should leave it at this, but only that this is not the place for an ultimate understanding of the dialogues. One further observation: if we assume that Plato's opinions, as opposed to those of Socrates, are expressed by such unidentified speakers as the Athenian stranger, etc., we will not be able to prove that these men consistently express a commitment to the absolute which is clearly different from the views uttered by Socrates.

[17] Consider, from the vast evidence of the dialogues, only these three crucial instances of the fundamental character of Socrates' ignorance. At *Symposium,* 177E, Socrates says that he is one *os ouden phémi allo epistasthai é ta erotika.* That is, he is a *lover* of wisdom, and never in possesion of it. At 200–202, it is explained that love is man's *epithumia* for what he lacks, for his completion. For a fuller discussion of the meaning of *eros* than can be given here, see my article, *Wonder, Anxiety and Eros* (Giornale di Metafisica: no. 6, 1957). At *Gorgias,* 509 A, Socrates explains that he does not have certainty, except insofar as it rests upon the inability of others to refute him, i.e. to teach him something better. His continuous quest is just this: to learn something better than what he knows. At *Apology* 20 D–21 D, Socrates states that he does not possess unqualified wisdom, but only *anthrōpiné sophia,* i.e. not *divine* wisdom, which is the knowledge of the Ideas. Note also at 40 C the explicit admission that he is ignorant about immortality and at 41 B that he would even continue his investigations in heaven. To take such a stand is the same as to deny the possibility of perfect wisdom.

[18] *Theaetetus:* 209 A–B. Knowledge is *doksan aléthé meta logou* (202 A–D). Note also that at 197 D, the forms of knowledge are represented by birds. We may recall that Aristophanes, in satirizing Socrates, uses the image of birds to stand for the Ideas.

[19] Consider *Timaeus* 47 A–B, 51 B–E. Intelligible forms are not perceived, but it is perception (and opinion) which leads us to infer by dialectic that there are Forms or Ideas. Again, we do not know first that the bed is a copy of the Idea of the bed, but we know of the Idea only by reflecting upon the bed. (See *Republic,* Bk. X.) And consider the account of one's progress, by dialectic, from opinion to knowledge in *Republic,* VI. 509 E – VII. 535. Compare 532–534 with the passage cited from the *Timaeus.*

[20] See *Phaedo,* 65 A–66 E. (At *Sophist,* 246 A, the Ideas are said to be *tén aléthinén ousian.* If the Ideas cannot be rationally, i.e. certainly, known, then Being cannot be certainly known.)

[21] One would have to understand the Platonic notion of myth in order to see how it is possible to combine the intuitions upon which philosophy rests with the problematic character of philosophy. It should not be forgotten that the *Timaeus* is itself only a likely story. I have discussed this question in a paper, "Clarity", which will appear in the *Giornale di Metafisica.* See also the previously cited "Wonder, Anxiety and Eros."

[22] *Phaedo,* 64 A; *Theaetetus,* 174 B.

[23] *Republic,* VII. 519 ff.

[24] See *Phaedrus,* 230 D, where Socrates explains that, as a lover of learning, he stays in the city. The dramatic structure of the *Republic* makes clear that, in order for philosophy to emerge, there must be a highly complex society and a wealthy class whose

sons have the leisure for study and conversation. Thus the conversation takes place in the Piraeus, the business and shipping center, and is bequeathed by the wealthy Cephalus to the younger generation.

25 In *Republic*, 596 A ff., Socrates implies that man makes the whole in the sense that a rotated mirror constructs an image of the visible universe. The soul is the mirror in which the Ideas are reflected.

26 The utilization of political laws is due to our imperfect knowledge and to the impossibility of governing circumstances consistently by knowledge alone. (*Statesman*, 292 C–300 E) Men are forbidden to change their laws because of their ignorance, and the dangers which follow from change, not because of any knowledge that one's laws are best (301 A). At 301 E–302 C, it is indicated that there is no absolute solution to the political problem (at least barring the unlikely advent of the philosopher-king); Conservatism is a substitute for political knowledge, or for the practical impossibility of implementing political knowledge. The same situation is illustrated throughout the *Laws*. Truth is distinct from utility (667 B); lying would be allowed if it were politically defensible (663 d–664). Note also the similarity between the persuasive prelude to the laws (723) and the noble lie of the *Republic*. Consider also the comparison by the Athenian stranger of their discourses to a poem (811 C ff.) and of the city they are describing to a tragedy (817 B). The *theoretical* significance of Plato's objection to change is that it leads to intemperance, which in turn brings about individualism, the great corruptor of philosophy.

27 Consider *Philebus*, 66 A ff., *Laws*, 631 C. See *Republic*, 428 B, where the *sophia* of the wished – for city is identified as *euboulia* (good counsel, prudence), and 430 E ff., where *sophrosuné* is shown to be the harmony of the whole city; i.e. the *limitation* of the worse by the better, and of the part by the whole. Moderation is taught obliquely by the *Republic* as well as explicitly: Socrates restrains the natures of Glaucon and Adeimantus (Glaucon is called timocratic at 548 and an erotic man at 474 D; Adeimantus is identified as a musician, i.e. an artist or poet, at 398) by making for them a moderate city. Thus he exemplifies dramatically how true statesmanship is in fact the true art of weaving (see *Statesman*, 279 A–283 B) – the weaving of souls.

28 The first step in this expansion comes when, like Glaucon (*Republic*, 372), we reject "the city of pigs" and insist upon some luxury. It is extremely important to observe that there could be no philosophy in the city of pigs (compare note 24); the preference for Sparta is an indication that philosophy and political virtue are by no means perfectly compatible. See also *Laws* 704 E, where the Athenian stranger warns against building one's city by the sea. (Aristotle makes the same point in the *Politics*.) This is both a criticism of historical Athens and a rejection of political expansion.

29 It is interesting to note the dramatic setting of Plato's two great dialogues on love, both of which have as their theme the critique of love as selfgratification. The *Phaedrus* takes place outside the walls of the city, and the *Symposium* at a drunken banquet. (Compare the remarks at *Laws*, I, 645 D ff., where drunken parties are said to be valuable tests of a man's capacity to withstand pleasure.) Alcibiades, the most striking figure in the *Symposium*, represents within the dialogues the result of excessive self-gratification, i.e. *individualism:* the corruption of a philosophical nature.

REPLY TO STANLEY H. ROSEN

by

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1. I took great interest in reading the reflexions evoked by the publication in "Inquiry" of the English translation of my exposé presented at the Warsaw meeting of the International Institute of Philosophy, concerning the theoretical relations of thought and action. My text covers, in a few pages, a vast field, a central one to philosophical thought; its interpretation therefore calls for both some attention and a minimum of intellectual sympathy. I am glad to see Mr. Rosen found in it an occasion to expound his own ideas, but I am not sure that he made the effort required to take effective cognizance of mine.

In my exposé, I contrast a classical philosophical tradition (whose most characteristic representative is Descartes) with the contemporary tendencies represented by philosophies as different as marxism, pragmatism and existentialism. The object of my perspective is to take the doctrines which assert the superiority of the eternal over the temporal, of contemplation and science concerning the immutable over practice, production or technique, the superiority of essence over act or existence, and contrast them with the doctrines which judge the Theory by practice, the principles by their consequences, and assert the superiority of existence over essence.

Mr. Rosen, identifying this opposition with the distinction between rationalism and empiricism, observes that following up my statements, one should situate Hume in the rationalist tradition. Mr. Rosen is surprised to see how much I put into my "rationalism", because — as he writes — "this would imply that empiricism is a form of rationalism." (p. 6)

I am making a point of stating clearly, in order to avoid further misunderstanding, that what I call the classical tradition, starting with Plato and Aristotle, continues with St Augustine, St Thomas, Duns Scotus, Descartes, Leibniz and Spinoza and is *carried on by empiricism and logical positivism,* as it is represented by early Wittgenstein of the *Tractatus Logico-Philosophicus.*[1]

This classical tradition includes all the philosophies which are satisfied with a definition of truth as conformity with the real and see in true knowledge only a reflexion of what is.

Mr. Rosen takes me to task for blending into this tradition Greek with Christian philosophers; he dwells, at considerable length and rightly so, on what differentiates them. He might be the more surprised to learn that, from more than one point of view, the great English empiricists, as well as a fair number of the positivists, equally belong to this tradition. In my opinion, it is not at all a question of confusing, but rather of applying a different principle of classification. I am sorry to have failed to clarify its terms sufficiently, seeing that it seemed possible to identify it with the classical opposition between rationalism and empiricism, whereas, as far as I am interested, I draw no essential distinction between them.

2. In order to make myself clear I shall restate very briefly that the tradition I called classical assigns but little importance, as far as achieving science and contemplation goes, either to practice or to the historical and situated aspects of knowledge. This tradition will recognize the role of practice as far as "doxa", opinion, is concerned, but in the field which is considered philosophically important it asserts a complete separation between valid knowledge and practice.

This viewpoint is held in common by Plato and Aristotle, as well as by thinkers such as Descartes. One has but to recall the Greek word *theoria* which means contemplation, and compare it with the meaning acquired by the word "theory" in modern thought, in order to understand the change in perspective to which I would like to draw attention.

The tradition I call classical includes all those who believe that by means of self-evidence, intuitions — either rational or empirical — or supernatural revelation, the human being is capable of acquiring knowledge of immutable and eternal truths, which are the perfect and imperfectible reflexion of an objective reality. There is nothing unusual in the fact that various thinkers of the classical trend would differ on a great number of points, nor in the fact that Christian thinkers present some particularities alien to Greek philosophy.

Socrates would be accepted by Mr. Rosen as a representative of the Greek tradition which, though asserting the superiority of the absolute value of reason, does not believe that the latter could furnish us with precise rules of action, for all knowledge according to this tradition is "open to continuous correction, permanently and intrinsically incomplete" (p. 15). This certainly is not the viewpoint of Aristotle, for whom no error is possible as far as fundamental truths are concerned.

It is desirable that an exact historical study should not only bring out differences between Greek and Christian thinkers but should also make an effort not to confuse various Greek philosophers between themselves.

3. In the second paragraph of his article, Mr. Rosen endeavours to show how nonsensical it is to try to make action provide a criterion for thought.

It all depends on the meaning attached to such an expression. The superiority of action over thought is, in my opinion, simply tantamount to refuting that the truth of a statement should stem exclusively from intuition, self-evidence or revelation, and to implying that elements furnished by practice, by decision and choice are involved in every knowledge. It is tantamount to asserting the superiority of *practical reason,* which implicates the refutal of an outright separation between thought and action.

At this point, Mr. Rosen's views are fairly akin to mine, and I could share his criticism concerning Hume's position, which Mr. Rosen is somewhat inclined to identify with my own ideas.

4. Mr. Rosen is at liberty to qualify as rationalist any position which attributes to the exercise of reason, i.e. to philosophical activity (and generally to scientific activity) superiority over any other human activity. It gives him the opportunity of contrasting the Greek rationalism with Christian tendencies, salvation of the soul being for the latter the essential objective of every person. It is permissible to characterize rationalism, as Mr. Rosen does, from an axiological viewpoint, whereas we usually qualify by this term an epistemological attitude.

I will merely insist, in this context, on the ambiguity of the meaning of *reason,* which may constitute the center of an absolute and dogmatic philosophy as well as of a relativistic and critical one.

My own purpose was to make a distinction between two great philosophical tendencies, neither of which, although diametrically opposed to each other, allows an adequate place to practical reason, seeing that the former slights the practice and the latter ignores reason. It seems to me that my effort goes, by a somewhat devious way, to join Mr.

Rosen's own. I shall end by expressing the hope that, if indeed lack of sufficient comprehension opposes us, more comprehension will be able to bring out what unites us.

NOTE

[1] See on the subject my article "De la preuve en philosophie" (of which the Hibbert Journal (1954) carried an English translation), published in Rhétorique et Philosophie, Paris, 1952, as well as: Ch. Perelman & L. Olbrechts-Tyteca, Traité de l'Argumentation, Paris, Presses Universitaires de France, 1958, pp. 1–3.

METHODOLOGICAL UNCERTAINTIES
IN POLITICAL SCIENCE*

by

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During the last few years, the interest in political science has developed greatly in Europe: henceforth it will be exceptional to find countries in which professorial chairs and institutes for research work on this subject have not been created. It can be considered certain that this development — which is to be found in the general expansion of social science — is far from having exhausted its power. It is reasonable in the near future to expect European universities to give a growing importance to the scientific analysis of political life.

Now, this evolution was accomplished under curious and even disconcerting conditions. In many cases, political science was recognized as a university subject much more out of the wish to follow the fashion than as a result of reasoned conviction. It is easy to observe that numerous Faculties or Schools have established teaching of this kind without knowing very well what was really involved in the subject: hence there were serious hesitations, especially about the criteria to be used in the choice of the professor and final adoption of solutions completely different from one case to another.

Such a situation is not the result of chance or ill-will; it reflects the methodological uncertainties of this subject. It can be stated that even in a country like the United States, where, for many decades, political

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* This article is a summary of the most important themes in a book recently published by the author entitled: *Introduction à la Science Politique.* Paris, Armand Colin, 1959. In order to abridge the text, it was decided not to insert the footnotes referring to the developments in the book. However, a bibliographical note to be found at the end of this study will provide for the reader the information essential for a more detailed examination of the matter.

science has enjoyed an autonomous status and the advantage of material facilities, studies of official reports of professional meetings cannot fail to reveal a striking variety of points of view expressed by American scholars on the meaning of their work. The confusion is still greater in international Conferences (for instance, those organized by the International Political Science Association).

It may be said that all social sciences have reached that stage to-day. This assertion is quite true. We think, however, that the inadequacy of methodology remains greater in the field of political studies than in other areas. Its advocates — American political scientists included — on the whole, have not sufficiently considered the conditions, demands and implications of the scientific analysis of the political world: in particular, up to recent times, they did not take into consideration the growth in other fields of knowledge (like anthropology or social psychology) in which the importance ascribed to the method was greater. Hence, amongst numerous specialists in political science, less emphasis was placed on scientific precision and less need was felt for the quality of the demonstration.

Since we are firmly convinced of the importance and of the possibility of a systematic analysis of politics, we think that the best service we can render is to enumerate these uncertainties as they are. Some will perhaps find arguments in favour of delaying the expansion of this science in university circles. But this negative attitude will have no influence if understanding the difficulties leads to an effort which will help to overcome them.

The object of this article is not to propose solutions of the various questions mentioned: we shall only endeavour to indicate the problems, not to treat them thoroughly. Our analysis, necessarily short, cannot be considered as exhaustive; we shall try, however, to bring to light the most characteristic points.

1. Field of political science

It is in the delimitation of the field that hesitation is manifest. What is the place of political science in the analysis of social life? There is no generally admitted answer. Let us try to circumvent the opposition by observing at the start that it is perhaps less strong in reality than it seems.

The traditional object of this discipline is the study of public life, or rather, of the administration of public affairs. In our time, many authors have criticized this conception, which they find too narrow, and

have proposed wider definitions. Thus Bertrand de Jouvenel proposed that political analysis should be entrusted with the duty of examining the conditions and modalities of organizations (and of disorganization) of various social groups ("agrégats à maturation rapide", to use his own expression). Another example of a broad conception is that of George Catlin, who identifies the field of politics with the analysis of the human being, in that he tries to force his will on others. Lastly, let us quote the case of Harold Lasswell who ascribed first importance in his deliberations to the distribution of "values" in a society.

These attitudes are not without interest, but if adopted they would have the result of encroaching upon the field of other social sciences and would probably lead to neglect of that which is peculiar to political life. Most specialists have remained faithful to the criterion of public life, but how can we express it in a form which gives it an operational character?

This question is important since, on the answer which is given, depends, on the whole, the orientation taken in the research work. The choice of a centre of interest has an influence in determining the type of problem that the scholars will set down in their programmes of work and teaching.

From this point of view, two great principles of definition have prevailed in succession: the State and the political power. To-day, the conception which makes of political science the science of the State loses importance and it seems preferable not to take it into consideration. The very concept of the State is disputed and ambiguous. Corresponding to a moment in the organization of human societies, this concept is of a particular nature. Lastly, and chiefly, it had the result of concentrating the analysis on institutional aspects (and in truth, formal) to the prejudice of the studies of behaviour. The definition of the subject as political power is certainly open to question: see the difficulties contained in the elucidation of the very concept of power. But it had the enormous advantage of increasing the positive studies of the forces which struggle to obtain power, to maintain it, to resist it; their social composition, the modalities of their action, the results of their proceedings.

Recently, some efforts have been made to give to political science a content which is to some extent functional. The most remarkable is that of David Easton, who defines the subject as the analysis of "authoritative" decisions. Two factors characterize this type of decision: those it concerns or affects consider that they must, or at any rate should, obey

it; the measure taken forces itself on all members of society, even if only some of them are concerned. We see that the adjective "authoritative" contains modalities of application, the respect owed to decision and not the process of elaboration (which may be democratic or dictatorial).

The merit of this definition is to direct the research work towards the study, most neglected up to now, of the decision-making process: its institutional forms, but also its social contents. It seems possible to say that all societies, in some way or other, possess a structure capable of making such decisions. One important point is the difference between societies (in the light of time and space) in the field of authoritative decisions: this creates the "flexibility" of the political sector. The effort of Easton was therefore useful; however, we may hesitate to make of the decisional analysis the criterion characterizing the political study in any type of society.

Since we are dealing with the question of a subject of such rapid development at this stage as contemporary political science, it seems difficult to consider that any principle of definition may be valid for ever. The only realistic way is to choose one as a tool of work and a unifying element. In this respect, the idea of "structure d'autorité" set forth by Raymond Aron seems satisfactory, whether one explains it either by the way it is exercised (political power) or by the product of its application, the "authoritative" decisions.

The remarks above prove that political science really does possess a subject-matter: even if they differ to a small extent in the choice of a formula for definition (and consequently hold different opinions as to how the phenomena should be approached), most specialists in this field are interested in the same subject. But a great many differences of opinion exist as to the method of further investigation.

2. Political science and political sciences

It is a famous quarrel. For some, the study of the sphere of authority (or of government) depends on a subject having its justification and its full autonomy in regard to the other subjects of social sciences. Others reason differently: this analysis should study, not one science only, but various subjects which, at some point or other of their development, necessarily meet with the problems of "structure d'autorité". We should thus have a political anthropology, a political history, a political philos-

ophy ... The conjunction of these efforts would constitute "political sciences", or, as some prefer to say, "political studies". The discussion is worth examination.

This conflict has not a univocal significance. For some, who are found amongst the representatives of disciplines traditionally established (especially, but not exclusively, lawyers), the idea of "political sciences" was and still is an argument against the introduction of political science in the University. Others have no intention of obstructing: they believe that political analysis could reach its aim better by gathering specialists (dealing with subjects that have often reached a very high standard). This point of view had and still has many advocates, especially in Great Britain (where many think that the expression 'political science', supposed to be of American origin, is too ambitious in view of the results obtained and the results obtainable: hence the success of the expression "political studies").

Let us declare frankly that this position should be criticized and seems even dangerous. The notion of "political sciences" is, empirically speaking, of interest in the countries where, for some reason or other, it is not possible to introduce political science in the University: it is, in this case, a compromise which enables the students to study in spite of all the "structure d'autorité". But the process presents one main weak point: the absence of unity amongst subjects of study which differ in their principal trains of interest. The study of public life is then — and on this point of view the French experience is absolute — like a mosaic. Another serious danger is the errors and excesses committed by specialists in some particular subject when they attack the field of authoritative relations. The wanderings of a form of political geography (a monstrosity called geopolitics) is the most striking example of such a deviation.

It seems that some specialised science is absolutely necessary to create the unification of studies of public life, to give them a principle of coherence. This conception does not mean establishing a "hunting reserve". We think, on the contrary, and shall state it clearly further on, that one of the conditions of the development of political science is to be found in a close cooperation with the other social sciences: one way of collaborating would be for the second to yield to the first some material concerning, directly or indirectly, political life. Without a subject of study having to deal with the global elaboration of these elements, it is likely that they would be incompletely and irrationally exploited.

The point of view of unity gains ground every day: even in Great Britain, many scholars declare themselves convinced advocates of political science. And reaching this point when specialists of this subject approve of and wish for this inter-disciplinary cooperation, the conflict loses much of its substance. To-day, however, a trend should be noticed towards differentiation which started this time in political science itself, especially in the United States: many specialists in various sectors of the subject (in particular, "international relations" and "public administration") demand to-day, with more or less violence, the concession of a status of autonomy.

We do not intend to discuss here the heart of this matter. We think that loosening the ties — and *a fortiori*, a complete separation — would be detrimental to the whole of political science and also to isolated ramifications. The best solution, as was successfully attempted in many places, would probably be to seek concrete arrangements which would bring out the complete value of each part of its subject. Such a system would be, at any rate, preferable to sterile disputes (too often the mere expression of personal rivalry).

3. The various types of political analysis

The example of the United States shows that it is not sufficient to constitute a unified and autonomic political science to raise doubts in the study of the "structure d'autorité". There exists in that country a controversy incessantly renewed, on the meaning and the final value of analysis. The ambivalence of the term theory, as soon as it is applied to political research, expresses well the basis of this controversy.

For some, the role of theory is to formulate prescriptions dealing with the government of human societies. The task of the specialist being to express what should be, it is his duty to specify both the ends and the means of public activity. For others, the object of the theorist is to study the functioning of political life and to give an explanation as complete and coherent as possible. The analysis deals with what exists: it takes the way leading from the observation and the classification of facts to the statement of generalisations and uniformities.

We think that these two methods of approach are legitimate; but we are also convinced that it is necessary to draw a clear line of distinction between them. If we wish to found political science on a solid basis, it must cease to fluctuate between moral expression and positive explanation.

To our mind, the object of theory is explanation, exclusive observation of normative judgements. From this point of view (the most used in social sciences), theory forms a whole articulated with definitions, assumptions and propositions about some definite subject. It should present relations capable of being verified. In the first stage, we find hypothesis; at the end of the research, we reach causal proof and prediction.

Up to now, political science has been but moderately interested in theoretical formulation. Description has been very much used, *(hyper-factualism)*, but generalisation less so. It is probably in this that the greatest of its weaknesses lies. A considerable effort should first be made to express particular theories relative to a definite order of fact (theories called *middle-range* by R. Merton), and then to prepare a general framework where they will logically apply.

Amongst the conditions of such elaboration, let us mention the necessity of a semantic codification. The communication and the comparison of the results form the elementary condition of the progress of theory. Now, these operations are made very difficult by the extraordinary variety (and imprecision) of the vocabulary of political science and the almost complete absence of uniform conceptual expressions. In this respect, this subject of study still remains in a "pre-scientific" state, which is most unfortunate.

But other "methods of approach" in political life are possible: especially reflection along the lines of the great synthesis of the past. The expression "political philosophy" is used extensively in the United States. This concept, however, is not clear. According to some, we should pass on to the philosophical plan as soon as normative approaches are applied: in a word, the political philosopher would have to deal with ends as well as means. It is not certain that philosophers should ratify such a declaration, which is rather elementary on the whole.

We could then attempt to separate the doctrinal elaboration and philosophical thought. This new step would be meditation on a particular category of phenomena as regards a specific philosophy: it would have a justification only according to the total system of the explanation of the universe in which it finds its place. The doctrinal elaboration would be distinguished from political science in that it may judge or may perhaps propose a way of reform: but it would branch off from philosophy inasmuch as, being sufficient unto itself, it would not be necessary to refer to a philosophical system. But this distinction thus pointed out is weak and contestable.

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At any rate, it seems preferable (and even strictly necessary), to separate the efforts which tend solely to positive explanation from those having another aim. Many uncertainties in contemporary political science originate in the ambiguity prevailing in this subject. Indeed, it is easier to draw the distinction on paper than apply it practically because of some personalities having the power of being effective successively or even simultaneously on both grounds: many useless controversies, however, would be avoided if everyone was clear in his statement.

The separation thus recommended does not tend to neglect the history of ideas in the training of the student and the elaboration of political science. Unlike many contemporary specialists, we think that a thorough knowledge of the great philosophical syntheses (as well as doctrinal, if this suggested distinction is accepted) of the past and of the present, is an invaluable factor in political culture. It is not true to assert that the authors of these syntheses always yielded to axiological or normative considerations: in many cases, they formulated positive generalisations and it is possible (though it would have to be proved) that many of them would retain their value for the present. On the other hand, the political scientist cannot eliminate the influence of ideas — of yesterday and to-day — without questioning the validity of the explanation he wishes to present.

4. Controversies on the spirit of explanation

Supposing political science has in view the theoretical and positive formulation as we have just characterized it above. In what spirit should political scientists undertake it? Some of the problems in question belong to general scientific methodology, others deal with the characteristics of the field examined. We should like to bring forward the views of both.

Amongst general problems, we shall mention first of all that of interaction between facts and theory. According to an elementary conception, the formulation of theory would occur once the facts are known and completely verified. Its improvement would proceed from the modification or substitution of the statement admitted by the discovery of new facts completing or contradicting the first collected notions. In other words, the research of the facts would take place out of the field of theoretical reference. Now, the most ordinary experience shows that theory occurs as an independent variable in the development of science. In scientific work, facts and theory are in constant interaction: contrary

to what some political scientists still think, it is impossible to undertake the collecting of facts without having in mind some outlines of the system, some principle of selection, in short some rudiment of theory. This assertion underlines the strategic part of hypothesis.

Another problem, particularly serious in political science is the temptation of monistic explanations. To isolate a factor and make it a central element, if not the exclusive one, of the theoretical formulation, forms a system of analysis which, in spite of the harm done, has always had and still possesses advocates. Generally, the monistic reasoning emerges into a deterministic interpretation of a more or less summary character. The recent effort of Karl A. Wittfoged to explain despotism in the countries of the Ancient Orient by the system of irrigation, is a good example of this bad method (which those who devote themselves to political science have sometimes failed to avoid).

It is useless to insist on this point. If political discipline wishes to obtain the status of a science, it must accept the demands of the precision of scientific method. But does the nature of the field covered allow it?

Many doubt it. Of the arguments which they bring forward we shall give our attention to that of the absence of direct experience and to that of the impossibility of sufficient objectivity. The first one does not impress us: it is true that most of the time the specialist is obliged, short of direct access to governmental circles, or to leaders of groups, to get information and to interpret from outside. But we have at our disposal many more data on politics than some are ready to admit: it is not acknowledged that what remains hidden is more important than what is accessible. This argument is often a justification for laziness or conformity.

The second argument dealing with objectivity is more serious. Even if he endeavours to avoid expressing all value judgement, is the political scientist in a position to be objective? Does not the selection of facts result from uncontrollable impulse? Does not the sociology of knowledge show us that all knowledge is judgement? This problem is common to all social sciences: the existence of a link of interaction between the observer and the observed is a constant preoccupation. Moreover, it can be declared that the nature of the problems treated and the intensity of the fights amongst parties make it especially acute in political science. But we must not be defeatists because of all that. The effort at impartiality of the scholar, even if he does not attain a degree of complete detachment, is a precious guarantee. Moreover, we are today in a position to plan the procedures of the research so as to reduce

the risks of a selection and of a unilateral interpretation and *a fortiori* polemics.

The arguments which we have just discussed often lead their supporters to recommend a way of analysing which attributes great importance to empiricism, to flexibility, to the "esprit de finesse". Some, using as a pretext unquestionable difficulties in political analysis, recommend a system of explanation which is akin to a sort of superior journalism. On the contrary, we think that in this respect, nothing distinguishes fundamentally political science from the other social sciences: like them, our subject must tend to precision (the problem of likenesses and differences between exact sciences is intentionally left out here, as no one can yet foresee the results that the latter will attain in a century or two). Rather than finding pleasure in pointing out such difficulties, it would be preferable for the technicians in political science to devote to the study of methods more time and energy than they have done so far.

5. The quarrel of methods of research

It is in great measure a false problem. Its origin lies in the habit adopted especially in France — with what impulse nobody knows — of discussing by what global methodology political science must be inspired: historical, geographical, sociological, legal, statistical.

Thus put, the question has absolutely no meaning.

Each of these "methodologies" is expressed by the means of some well-known methods, in variable proportion: the analysis of documents, observation, experimentation, comparative study. Each is working by the medium of technics of research always improved to achieve the optimum results from the chosen method. These technics offer qualitative material to suit each case, or mathematical indications which can be presented and treated with the tool of statistics (which is applied to all fields of knowledge and belongs truly to none). Political science must not choose, for instance, between sociological or psychological method: the question for it is to know in what measure and by what technics it will appeal to such or such great categories of the proceeding of scientific analysis.

These trite observations (that some will hold as expressions of the commonplace) are necessary, considering the intellectual confusion which is to be found in the matter. In the countries where it has just been introduced, political science has not yet at its command any body of specialists: it must, then, at the start, appeal to technicians of adjacent

subjects (or subjects which are considered such), who are prepared to devote themselves henceforth to systematic analysis of political life. Now, they have a tendency to transpose mechanically onto their new subject the habits of work they acquired in their previous training. This desire of sudden transposition is unfortunate: it is particularly obvious amongst lawyers, who, more or less everywhere, wish to take up political science and whose dogmatic intolerance often (but certainly not in all cases) causes a very serious danger to the future of these studies.

The choice of the optimum method evidently depends on the nature of the problem presented and of the sources available. On many occasions we have to turn to documents: however, direct observation on the spot is an approach more and more used. On the other hand, experimentation in its epistemological sense (observations made in special conditions to verify a hypothesis) is rare. The difficulty of isolating a factor to provoke its variation is particularly great in the political field. This situation is not hopeless: it has been possible to establish some very precise subjects for matters where experimentation is either unconceivable (astronomy, geology), or not easily feasible (anthropology). Hence the importance of comparative analysis: particularly usable in political science, this method constitutes for it a substitute for experimentation (as Durkheim has shown it to be for all social sciences). Moreover it is necessary to proceed to a real comparison: now, in most cases, the so-called comparative studies (comparative government) are, according to a severe but correct judgement, essentially non-comparative.

During the last few years, technics of research have advanced constantly towards greater precision. Thus, the procedures of quantitative semantics and content analysis, of which American political science makes intensive use, have been applied to literary analysis of documents. Besides, the method of public opinion polls (which benefits the observation of characteristics by statistical sampling) now constitutes, in spite of all its faults, a way of examining political phenomena which cannot be replaced. We also know that the method of interview has been continually improved and has been the object of many attempts at codification.

It is unnecessary to give more examples. Important instruments of precision exist of which it is desirable for political science to make great use (without losing sight of the somewhat delicate character of their use). We shall not inflict upon the reader the refutation of the arguments of the traditionalists against the use of these new technics: at the beginning of the era of the railways, there were also people who

declared themselves against that mode of locomotion and predicted its failure. Inversely, we must not fall into the excess of the neophytes who subordinate the choice of the problems to be treated to the state of the technics available. The elementary rule is to start from the problem and then to choose the most appropriate means for its study (if need be attempting to bring in the necessary improvements).

One of the most interesting aspects of many of these technics is the substitution of figures (some say measure) for literary expression. But this is a particular aspect of a more general question.

6. Mathematics and political science

The use of mathematics in political science is one of the aspects of the present methodological discussion. It is not unusual. All social sciences have discussed the same point and the fact that ours has avoided it for a long time is due only to delayed development. It would be all the more desirable to look at the problem again thoroughly, as to-day a great evolution is taking place in mathematics itself.

For a long time, specialists in social sciences have been inclined to identify mathematical analysis with quantitative research: in this aspect (corresponding to the use of traditional mathematics), the assistance to be expected, if it is valuable, remains limited. But now new forms of mathematical thought appear which have a tendency to express qualitative phenomena (such as the theory of games) in precise formulas. Now, many subjects (linguistics, anthropology, economics . . .) seem to have benefited greatly by the contact made with these new speculations. Why should political science not imitate them?

The help given by traditional mathematics to political science has consisted mainly in the use of the resources of statistical analysis (from the simple technic of correlation to the subtleties of factorial analysis). Many ideas in political study are given directly in figures: election statistics, for instance. On the other hand, some modern approaches (analysis of the content and of the polls in particular) enable us to express quantitatively some phenomena considered so far as being qualitative. There was *a priori* no reason for not submitting this material to statistical treatment: it has often been done and in many cases interesting results have been obtained. But a great part of the data necessary to political explanation cannot be reduced, for various reasons, to a statistical presentation. And above all, without any factor of unification (playing the part, for instance, of money evaluation in economics), it

is not possible to pass from one element of analysis to the other except by reasoning in words.

This obstacle might have been avoided by creating a unity of the measure of power or of influence. But this problem has not gone beyond the stage of preliminary study. Moreover, we must agree on the meaning of such a measure and specify that the so-called operation (especially by social psychologists for the study of attitudes) is completely different from that obtained by, for example, the meter. The evaluation of an attitude is of ordinal nature; if we admit the basic conventions it requires, the observed subjects can be classified, some in relation to others, according to a particular criterion. But this method does not supply any "zero point" which would enable us to obtain a cardinal measure: according to logical reasoning, it does not go beyond the approach of the professor who estimates, on the day of examination, the knowledge of the candidate, or of an animal psychologist who evaluates the intelligence of a mouse.

If we had not gone any further, it would not be untrue to say that the main point of political analysis escapes mathematical reasoning. But do not modern mathematics try to avoid the diptych summary quantitative-qualitative and, as far as we are concerned, try to go further than the stage of quantitative scales with a point zero and equal intervals. The characteristic feature of this effort seems to be an extraordinary flexibility in the choice of the symbols and axioms. Henceforth, are not mathematics considered as axiomatic, transposable from one sector of research to another and are they not capable of expressing, in a very precise language, observations concerning social behaviour? Technicians do declare it possible, whatever the nature of the phenomenon in question may be, as soon as the propositions are coherent.

Hence the conception, which begins to take form, of mathematics in the sciences of man: a conception still extremely vague, but the first applications of which seem full of promise. They are numerous: from the introduction in the science of economics of the considerable experience proceeding from the analysis of the electric system, to the interpretation of groups as system of equilibrium (amongst which any modification of a variable brings on changes in the others until a new point of equilibrium is found). In other directions, we know what use has been made of the theory of the games of strategy by many branches of knowledge.

No one can foretell with certainty what are the contents of these "mathematics of man" and the advantages which will spring from them

for the specialist in social sciences. But political scientists would run a serious risk if they continued to neglect this movement as they have done so far.

The main reason for this absence of interest lies in the almost exclusively literary character of the training of these technicians. Today many arguments against the use of mathematics are really "rationalisations": everything about this way of reasoning being unknown, one believes that one can just declare it useless and even harmful in the field in question.

This childish attitude towards statistical analysis has vanished by degrees (very slowly). Progressively one has come to the idea that a minimum of mathematical culture is necessary to the student and, *a fortiori,* to the scholar in political science. But the new currents of mathematical thought cause more complex problems as knowledge of them generally demands a very high technical standard. Henceforth, how can we appreciate the respective value to political analysis of traditional logic and of this symbolic logic which is procured by mathematics?

Amongst the present uncertainties of this subject, the latter is not the least. Probably the best solution at first would be that mathematical specialists should become interested in the study of the "structure d'autorité" and should test the value of their instruments of reasoning. This collaboration between them and political scientists is not the only one which is necessary to-day to political science.

7. Political science and social sciences

We shall not here permit ourselves the luxury of commending to those few who are still behind the times the necessity of a cooperation between social sciences: who can question its urgent need to-day? We should only like to show that it is particularly necessary to political science. And from two points of view.

First of all, inasmuch as their effort on methodological thought has been insufficient, specialists in this subject must borrow a great many of their methods and their tools of study from subjects more advanced from this point of view (particularly sociology and social psychology). On the other hand, political science finds it necessary to take from other subjects a great many data. This point must be underlined, as the contents of political interpretation are necessarily enriched by it.

Limited to the study of institutions (an orientation greatly appreciated by lawyers), it can, in a way, be sufficient unto itself. But it gives only

a very partial view of the phenomena studied. By degrees, political scientists have introduced into their deliberations the game of social forces and, more recently, the influence of individual motivations (those which localise themselves in the unconscious mind included). By this, they are interested in some aspects of phenomena, the general study of which depends on other subjects of social sciences. It is desirable that political science should no longer limit itself (as is often the case nowadays) to borrowing passively such data, but should share in their elaboration: this desire, however, should not go as far as claiming a complete autarchy (the political scientists themselves dealing with all the data which have become absolutely necessary to them).

Thus, political science is obliged to keep up intimate relations with many subjects, such as psychology, sociology and the science of economics (the saying of Charles Beard should not be forgotten: that by neglecting economic facts, it would only succeed in going beyond the standard of astrology). Without insisting any further, being short of space, we shall say that this subject has also the greatest interest in following closely the works devoted to societies different from ours: differences founded, either on time (history) or on cultural heterogeneity (anthropology, of which we also know to-day, especially in the United States, that it is no longer limited to the study of the "primitifs").

This situation does not belong to political science only. All social sciences know such reciprocity in respect of the communication of technics of research, such as the gathering of basic data. But, for the reasons we have already mentioned, political science receives to-day more than it gives: we cannot establish as a principle that it will always be so.

In our opinion, there does not follow any loss of independence. This subject keeps the responsibility of the explanation of one sector of social relations, even though it borrows much to accomplish it. This point of view, let us point out, is not accepted by everyone. Thus, various sociologists consider it as a simple branch of sociology: there exists, in the United States, a vigorous "political sociology" (which has obtained important results in many problems of public life: electoral behaviour, bureaucracy, participation in political life, study of extremism ...) At another level, those who support the "theory of action" (T. Parsons and E. Shils) contest the necessity and the logical character of a specialised theory of political relations. But does sociology really know what it wants? One point is certain: in no manner has it managed to constitute this general *corpus* of social knowledge that Durkheim and Simiand meant to make of it.

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Besides, let us note that the very existence of sociology is called into question by the most extreme supporters of behavioural sciences. This movement, full of promise, is much too recent to enable us to appreciate its value. Up to now, it has manifested itself in a less "revolutionary" manner than its supporters claim. It had a direct effect on political science by bringing on the birth of a behavioural school, which means to explore the political conduct about personality and the psychological motivations of participants. This school, which is remarkable for an exceptional precision in the choice of the themes of research and the tools of study, has obtained so far only limited results.

8. Applications of theory

They remain, for the time being, very modest. Any effort of serious application implies an attempt at prediction (of the type if ... then). Now, it is unfortunately clear that, generally speaking, political science has not reached that stage. Except for the predictions of electoral results, often most imperfect, (but is this proceeding a true prediction?), political theory offers few possibilities of examining the future on the basis of scientific considerations. For the time being, the field depends mostly on the "flair" of the political man or the journalist.

This explains the reluctance of government officials to consult specialists, the hesitations of the latter to take sides. These attitudes are conceivable and after all legitimate. This position, however, is not universally accepted. In the United States, the political scientist agrees and even asks to participate in social engineering: during recent years, Harold Lasswell has defended with great ability the formation of "policy sciences".

In spite of the serious lacks and insufficiencies in his subject of study, the technician can indeed help by giving his opinion about the methods of reaching the desirable aims (as happened, for instance, in many cases in the plan of technical assistance granted to underdeveloped countries). But if he wishes to make himself useful and to avoid the main errors, the expert must constantly keep in mind the limitations of the knowledge on which he founds his views.

By improving itself, political science will no doubt open to government officials possibilities of greater rational action. But what use will they make of it? Certain contemporary examples cause great anxieties, for instance, influence on people's minds by an effort of rationalised propaganda and the application (rather recent) of technics of "motiva-

tional research" to electoral operations. Shall we succeed, as the firm of *Whitaker and Baxter* in the United States maintains, in having a candidate elected as one sells soap or perfume? It is not certain: the fact of having at their command the services of powerful specialised firms in public relations did not prevent the Republicans from being badly defeated in the last congressional elections (November 4th 1958). But if it could be managed, even on only a small scale, what would remain of democracy?

At this stage it is necessary to reinstate in the picture the very aims of authority. The study of politics might reveal itself as decidedly dangerous one day (like many other branches of social sciences) if it were not completed by a political philosophy of man. Many times, we have insisted on the necessity of separating positive explanation (the role of political science) from thought on the place of man in the social system (belonging to the philosopher and even to the moralist): but we have equally asserted the legitimacy of philosophical thought. The discoveries of scholars and the way they are applied, remain, finally, dominated by the conception of man which prevails in the society in question. The established social philosophy will suggest how to use the discoveries of the technician and will give them a true human significance.

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To make this article relatively brief, we have dealt with the subject in summary form, which is necessarily incomplete and fatally allusive. Our object has been attained if the reader has had the feeling of the innumerable problems that contemporary political science contains, and the great efforts which will be necessary to solve them. But the importance of the aim fully justifies the efforts: in many respects, the political sector commands all the others. If we know nothing of its action, if we have only an impressionistic view of it (as is too often the case to-day) we are condemned to grasp most imperfectly the mechanism of human societies. Is it an acceptable situation in a world on the verge of conquering space?

Translated from the French
by *Evelyn Guisan*

BIBLIOGRAPHICAL NOTE

The basic work on political science within the university structure and problems of instruction in this subject, is still: ROBSON (William A.), *Les Sciences sociales dans l'enseignement supérieur. Science politique.* Paris, 1955. The documentations of this book are somewhat outdated, but the comprehensiveness of the view presented is still of great value. In the same series one may also consult two volumes which are representative of the present non-coherence of the subject matter: MANNING (C. A. W.), *Les Sciences sociales dans l'enseignement supérieur. Relations internationales,* Paris 1954, and MOLITOR (André), *Les Sciences sociales dans l'enseignement supérieur. Administration publique,* Paris, 1958.

On the present state of political science the most interesting work is still: EASTON (David), *The Political System. An Inquiry into the State of Political Science,* New York, 1953. The investigations of Easton, not always easy to read, are invaluable for their review of the present situation: the essay of reconstruction is fragmentary and in parts controversial. The small book by BUTLER (David E.), *The Study of Political Behaviour,* London, 1958, strikes another note. Very elegant in composition, this work faithfully describes the reserved attitude of British political scientists to the complex terminological apparatus and methods of measurement from which American political science is forged.

Concerning the same aspects one may mention the publication of Unesco: *La Science politique contemporaire. Contribution à la recherche, la méthode et l'enseignement,* Paris, 1950. This work retains an interest as a handy reference. Furthermore, some of the studies concerning the situation of political science in various countries merit close reading (in particular the one by Raymond Aron on France, the one by Thomas I. Cook and Benjamin E. Lippincott on the U.S.A., and the one by William A. Robson on Great Britain). In spite of the title, this publication is not so informative on the methodology of the subject. A serious gap here has been filled by the recently published manual by Maurice DUVERGER, *Méthodes de la science politique,* Paris, 1959. For the application of statistical techniques to the analysis of political phenomena, reference may be made to: KEY (V. O., Jr.), *A Primer of Statistics for Political Scientists,* New York, 1954.

AN INQUIRY INTO THE CONCEPTS OF 'RELIABILITY', 'INTERSUBJECTIVITY' AND 'CONSTANCY'

by

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1. Introduction

As it is one of the aims of all of the factual sciences to find invariances in the world around us, the possibility of assessing whether an *object* is the same or has changed seems to be of crucial importance. To do this, some kind of an *instrument* is needed, whether it is the telescope of the astronomer, the questionnaire of the sociologist or the "discerning eye" of the historian. But even though we apply the most perfect self-recording instruments, there must always have been somebody there to install them and to observe them. Not everybody is competent to do this — it requires training ranging from the training necessary to measure the height of a conscript to the training necessary to measure traces of infant experiences by means of psychoanalysis. We shall call people who are able to make use of the instrument to observe the objects *testers* (a term we prefer to the term "observer").

This interplay between object, instrument and tester will result in an observation, but we shall prefer to call it a *measurement,* implying by that term any kind of single-valued mapping of an object (by means of the instrument and the tester) on a (one- or multi-dimensional) set of numbers. Further, by using the term "measurement" we make no choice between the four traditional levels of measurement: nominal, ordinal, interval and ration (or any of the intervening levels).

It is this interplay we want to analyze with regard to how it is possible to determine constancy of an object. Since this is a problem of some importance in general methodology, we shall not tie the exposition to any specific field of scientific investigation. But the immediate field of application will be to sciences of "medium softness", e.g., the social sciences.

The problem is by no means new. To arrive at any judgment at all about constancy or stability of an object, more than one measurement of the object must be made. As it is impossible to measure it (obtain measurements) twice at the same time by the same observer (not even the split-half method in test-psychology does this), the two measurements must be at different points in time. The measurements can then be compared and a decision be made as to whether they show the same result or not, and this will be a result at *the manifest level*. But that is not sufficient for any inference about the object, because properties of the object are not given to us except through the mediation of the tester and his instrument; they are hence at *the latent level* in Lazarsfeld's sense (4, pp. 19 f), and can only be inferred.

Let us introduce symbols: for the objects (O), the instruments (I), the testers (T) and the measurements (M). We assume that O have been selected according to a criterion of *relevance* for our substantive problem, that I have been selected according to a criterion of *applicability,* and that T have been selected according to a criterion of *competence* which is independent of this particular measurement. As an illustration of the relationship between the four, let us have

$$ T \gtrless I \qquad \gtrless O $$
$$ \downarrow $$
$$ M $$
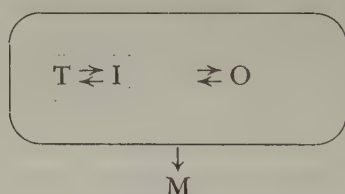
First of all, we look at T and I as a unit applied to O, where (T, I) is an extension or refinement of T designed for the purpose. In some cases, there are sharp border-lines between T and I, at least as we are used to looking at them. We can tell where the man ends and his yard-stick starts — if we do not take into account that a large part of the yard-stick is within the man, his know-how. But in the cases of the psycho-analyst and the historian we would not even talk about boundaries between T and I. The distinction between T and I becomes completely analytical, where I is T's use of the instrument as well as the instrument itself, and T is the part of the person that is not directly engaged in the measurement process. It is indeed a "fallacy of misplaced concreteness" to conceive of the borderline between T and I as coincident with T's body. For instance, in the case of an informal interview, this

is obviously not only an interaction between T and O, but an interaction where T has an instrument built into himself, viz., "the art of asking" and the results of the phenomenological analysis that has preceded the interview.

Secondly, it seems fruitful to regard the fusion of T and I as a process that may change I, and may even change T, as certainly is the case with the person who becomes a psychoanalyst. And thirdly, the process where (T, I) tries to get a measurement out of O is certainly a process that *may* change O (as in the case of interviewing), and may also change (T, I) (if not for other reasons, because of added experience). The double-arrows in the figure are intended to catch all these mutual dependencies.

All this said, it becomes clear why the process of making any inference from M alone to O, I and T is so difficult, especially when the measurements have to be made at different times. During time, T, I and O may or may not remain constant, and this may or may not show up in M. In special cases, as when we are measuring lengths with constantan or some similar alloy, we would say that changes in M must be due to changes in O — but in the general case the interplay may be such as to make them all change but still make M constant from t_1 to t_2. And changes in M may be due to changes in I only.

Let us now assume that we repeat our measurements under perfectly controlled conditions, where the total web of relevant conditions is either made irrelevant, kept constant, or the measurements are properly randomized. We only make two exceptions: we let the *time-distance* between first and second measurement vary, and assume that we can make a distinction between *short* and *long* intervals. This distinction is of course highly conventional and will vary considerably: in the life-history of a yardstick one year is probably a short interval, but it is a long interval in the life of an attitude pattern. In empirical work, this distinction is very important.

The second exception has to do with the *testers*. In most social science measurements, the tester-personality is a highly relevant condition, and when we have to do with two consecutive measurements, it becomes relevant whether they are made by the same tester or by different testers, where by "different testers" we mean two testers of whom it is not *a priori* known that they are completely equivalent as conditions for the measurement process. For instance, in the case of psychoanalysis, it is quite conceivable (though empirically not easily demonstrable) that two different persons have the same psychoanalytic theory as an instru-

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ment, but influence the same object so differently that the outcome (M) is different. In survey research, this is known as "interviewer effect".

These two conditions, we assume, are relevant for whether or not the objects, instruments and measurements are constant. Thus, we have dichotomized all our four variables and time-distance, and have let this suffice for the definitions although some interesting intricacies are always left out when continua are dichotomized. To discern and disentangle the interrelations or our variables we combine *time* and *tester* in a four-fold table:

Table 1. The basic four-fold table

	Tester same	Tester different
time-interval short	1	3
time-interval long	2	4

We get four sets of conditions which we shall refer to as quadrant 1, 2, 3, and 4 cases respectively, and under all four conditions we are interested in passing judgments on the constancy of I, O, and M. For this purpose, we introduce some proposition functions.

Let $I_{s,s}$ stand for the proposition "I is constant for short intervals and for the same tester," and correspondingly, let $M_{l,d}$ stand for "M is constant for long intervals and different testers," etc. All together, we get three sets of propositions and they fit into Table 1 as follows:

Table 2. The constancy propositions

	Tester same			Tester different		
Time-interval short	$I_{s,s}$	$O_{s,s}$	$M_{s,s}$	$I_{s,d}$	$O_{s,d}$	$M_{s,d}$
Time-interval long	$I_{l,s}$	$O_{l,s}$	$M_{l,s}$	$I_{l,d}$	$O_{l,d}$	$M_{l,d}$

To sum up: We assume that we can decide on the basis of previous experience, analysis and hypotheses whether the time-interval shall be

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considered "short" or "long". Further, we assume that we can find out by observation, introspection, and/or empathy whether the same tester is constant — and we assume, of course, that we know whether we have to do with the same tester or different testers. In short, we know whether we have a quadrant 1, 2, 3, or 4 case. On the basis of comparisons of series of measurements a decision is made concerning the M-proposition. This is necessary, but not sufficient (we cannot determine two independent variables on the basis of one alone) as a basis for a conclusion concerning the I- and O-propositions in the quadrant, which is our ultimate goal.

2. The definitions of "reliability", "inter-subjectivity" and "constancy"

The concept of "reliability" has a long tradition in social science and is obviously closely related to our problem, whatever definition we choose to make use of. The difficulty is, however, that the definitions often fail to distinguish between what is manifest (the measurements) and what is latent (the objects and the instruments), although most of them agree in making constancy or stability a part of the *definiens* for at least "perfect reliability". Most commonly, the concept is tied to the measurements alone, though it is often expressed that it has more to do with the instrument than with the object. But we also find the following from Newcomb (5, p. 159):

> "Sources of unreliability, when a test-retest technique is used, may lie in the persons who respond to the scale, to the degree that their attitudes change between test and retest"

where what we call inconstancy of the object is seen as a source of unreliability. This seems unfortunate, as one cannot possibly blame what one measures for changes that may possible be very interesting. Sometimes a distinction is even made between "instrument-reliability" and "object-reliability".

Guilford, in his important *Psychometric Methods* (3, p. 350) defines reliability as "the proportion of true variance in obtained test scores", where a distinction is made between the true score one would obtain if the measurement were accurate, and the score one does obtain, which is conceived of as the true score + an error component. The reliability is perfect if and only if the latter is 0. We shall, however, not make any use of "accuracy" and "error", but prefer to use reliability as a property of the instrument alone:

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D e f i n i t i o n 1: An instrument is *short-time reliable* if it is constant over a short time-interval for the same tester.
An instrument is *long-time reliable* if it is constant over a long time-interval for the same tester.

In symbolic form:

Short-time reliability: $\vdash$ $(i)I_{s,s}(T_i)$
Long-time reliability: $\vdash$ $(i)I_{l,s}(T_i)$

In the symbolic definition the importance of instrument-constancy not only for one person, but for all persons in the competence-group of testers has been emphasized. In practice, reliability of the instrument will very often be used as an important competence-criterion in the selection of testers. In many cases, however, reliability may legitimately refer to one person only, and we would substitute an existential quantifier for the universal quantifier. What we here call "reliability" may also be called "intrasubjectivity".

D e f i n i t i o n 2: An instrument is *short-time intersubjective* if it is constant over a short time-interval for different testers.
An instrument is *long-time intersubjective* if it is constant over a long time-interval for different testers.

In symbolic form

Short-time intersubjectivity: $\vdash$ $(j)(k)I_{s,d}(T_j,T_k)$ $j \neq k$
Long-time intersubjectivity: $\vdash$ $(j)(k)I_{l,d}(T_j,T_k)$ $j \neq k$

In the symbolic definition the importance of instrument-constancy not only for one pair of persons, but for all pairs of persons in the competence-group T has been emphasized. In practice, intersubjectivity of the instrument will very often be used as another very important competence-criterion in the selection of testers. It is not enough that a tester shows reliability in the use of the instrument; he must even be able to use it in the same way as a tester with a high reputation for competence. In many cases, however, intersubjectivity may legitimately refer to one pair of testers only (and we would use a weaker definition with $(Ej)(Ek)$).

Both reliability and intersubjectivity refer to properties of the instrument, where this word as usual is taken in the extended sense so as to include the testers' *use* of what we habitually call the instrument.

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The use of a yardstick is commonly regarded as both reliable and inter-subjective; the passing of a sentence by a judge is short-time reliable within reasonable limits, but hardly long-time reliable — and not at all intersubjective. But these questions are empirical and logical questions, and we have not yet developed the logical machinery we need.

Intersubjectivity is often called reliability, too, but we feel that the concepts are so different that they should be kept apart by different words. In most cases, inter-individual constancy in use of instruments (intersubjectivity) will be more difficult to obtain than the intra-individual constancy (reliability) — because so many relevant conditions change when we pass from one person to another. Another term for intersubjectivity is "objectivity" (14, p. 43), but we feel that this term should yield to the term "intersubjectivity" because it expresses far better the idea behind the term.

D e f i n i t i o n 3: An object is *short-time constant* if it is constant over a short-time interval for the same and for different testers.
An object is *long-time constant* if it is constant over a long-time interval for the same and for different testers.

It may, however, be useful in some cases to split the definition in two cases: "same tester" and "different testers". What is often called "interviewer-effect" is exactly short- or long-time inconstancy for different testers. Whether we should say that the object has changed due to the interaction process with a new interviewer, or that the interviewing is not intersubjective will often be difficult to decide. In practice, we will perhaps make the decision on the basis of some hypotheses about the influentiability of the object as this is known from other data. It is important not to confuse this case with the case of inconstancy of relevant conditions. If something politically important happens between two interviews, measurement-inconstancies need not be due to lack of intersubjectivity or lack of object-constancy for different testers, but may simply be due to a change in the object caused by his new perception of the political field.

All the definitions we have introduced imply the notion of measuring an object more than once. If the measuring process itself is a relevant condition for the object, or the object is a system with memory (e.g., a human being) the repetition may be invalid (as when an IQ test is

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repeated with the same problems after a short interval. A substitution of instruments may be necessary, and this requires the concept given in the following definition (7, p. 43):

Definition 4: Two instruments are *congruent* if they give the same results when they are applied to the same objects under the same conditions.

"Congruency" is an equivalence relation in I and can be used to partition I in subsets of congruent instruments. In practice, however, this relation is more likely to be a similarity relation if one will have classes of congruent instruments with more than one element. The concept "parallel tests" in psychological test theory is an example of a congruency relation. Of course, the definition can be split into the four cases corresponding to the four quadrants.

It is important to remember that short-time and long-time reliability and intersubjectivity of *instruments* are *desiderata*, whereas all the different kinds of constancy for the *objects* are *empirical findings*. The first is judged in terms of good and bad, the second in terms of true and false. Further, the definitions we have given are not operational; they are purely nominal definitions. In order to give them empirical content, we must show how it is possible to infer from manifest constancies in the measurements to latent constancies in the instruments and the objects. This will be the problem of the next two sections.

3. The logical relations between instrument-, object- and measurement constancies

Our task will be to infer from *one* manifest variable, M, to *two* latent variables, I and O — from the constancy or inconstancy of the instrument and the object. This would be impossible if it were not for two factors:

1. I, O and M in the same quadrant are not logically independent, and
2. it is possible to make assumptions about the constancies of I and O and the relations between them.

We shall deal with no. 1 in this section and with no. 2 above in the next section. Another way of expressing the same distinction is to

distinguish between *within-quadrants* and *between-quadrants* relations between the comstancies. As the four quadrants obviously can be dealt with in a quite symmetric fashion, we select quadrant 1 as typical and want to find the logical relations between $I_{s,s}$, $O_{s,s}$ and $M_{s,s}$ — i.e., the relations that are independent of the empirical nature of I, O and M.

The relation turns out to be somewhat complex. Part of it can be expressed as follows: If both instrument and object are short-time constant for the same person, the measurement *must* be short-time constant for the same person, as there is nothing else that can cause it to vary (we have assumed that relevant "space" is constant). *But the converse need not be true.* If the measurement is constant, no valid conclusion can be drawn about instrument and object, because the constancy *may* result from constancies in both, but it may also result from balancing inconstancies in both instrument and object.

That the latter is not only a theoretical argument may be seen from some examples. Imagine an interviewing situation where people's attitudes to an emotional issue are investigated by means of a panel study, and the interviewer at some point has to make an inference about a respondent's attitude to an important symbol on a scale running from "strongly unfavorable" to "strongly favorable". He might score the respondent as "favorable" or the first wave, and may give him the same score for the second wave because both respondent and interviewer have changed equally much in the meantime, so that the respondent's attitude looks the same judged from the new origin of the interviewer's coordinate system. Of course, one tries to avoid such a very elementary error by leaving the coding to some other persons, testing whether the coding is intersubjective among persons with different attitudes to the issue, etc., but the situation is very real.

Another example of a different nature can be seen in the case of testing the tensions in a person's muscles, by means of palpation. To discern tensions, the tester must be very sensitive, relaxed in his own muscles and alert. But if he encounters a case of extreme tension, it may both for psychological and physiological reasons be difficult to maintain this prescription, and a change to a more tense position during the palpation may result in a similar dislocation as above of the point of origin for the recording. As this is not merely attitudinal, it may very well be that different testers may undergo the same transition.

Exactly what kind of reasoning can we make with this ambiguity in the relation between the three kinds of constancies? To get a complete picture, we shall resort to truth-value tables. The three propositions can

all be true or false, which gives us $2^3 = 8$ possible combinations. Some of these combinations will be incompatible with our insight in the relation between I, O, and M; some of them will be compatible, and some of them may be both. Compatible will be indicated as true (t) and incompatible as false (f). This method will give us the very few out of the $2^8 = 256$ *a priori* possible truth-functions that express the relation between the three propositions — and we shall call these truth-functions S_1 ("1" for quadrant 1).

S_1 is defined by this truth-value table:

Table 3. — The truth-function S

	$I_{s,s}$	$O_{s,s}$	$M_{s,s}$	$S_1(I_{s,s}\, O_{s,s}\, M_{s,s})$
1.	t	t	t	t
2.	t	t	f	f
3.	t	f	t	f
4.	t	f	f	t
5.	f	t	t	f
6.	f	t	f	t
7.	f	f	t	f,t
8.	f	f	f	f,t

The difficulty mentioned above concerning inferences is rendered symbolically in the two last rows. We see that S_1 excludes 252 out of the 256 possible truth-functions, and thus limits the number of possibilities to $1/64$.

The three remaining quadrants will give us the other truth-functions, but they will be extensionally just the same. We call this truth-function all quadrants have in common S, and *assume in the following that S is true* — i.e., we assume that it correctly expresses the relations between the constancies of instrument, object, and measurement respectively. This means that we at once rule out possibilities 2, 3 and 5 as possible combinations, and thus limit our universe of possibilities from 8 to 5.

We see that the premise "$M_{s,s}$ is true" still does not warrant any unambiguous conclusion, though it excludes possibilities 4, 6 and 8. According to combinations 1 and 7, the remaining ones, both $I_{s,s}$ and $O_{s,\cdot}$ can be both true and false. But if we can add as a second premise the truth of either of them (and thus exclude possibility 7) the truth of the other follows because only possibility No. 1 is left. The inferences can be expressed as follows:

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$$\vdash \text{S} \qquad\qquad \vdash \text{S}$$
$$\vdash \text{M}_{s,s} \qquad\quad \vdash \text{M}_{s,s}$$
$$\vdash \underline{\text{I}_{s,s}} \qquad\qquad \vdash \underline{\text{O}_{s,s}}$$
$$\therefore\ \vdash \text{O}_{s,s} \qquad \therefore\ \vdash \text{I}_{s,s}$$

This is a formal proof for the intuitively very plausible proposition that we can infer from the constancy of the measurement and the instrument or the object to the constancy of the object or the instrument. The symbolic proof is perhaps unnecessary, but of significance in the following. We have not succeeded in expressing S by means of the conventional logical connectives in an intuitive way.

If we negate the second or the third of the premises, the conclusion will come out with a negation sign — as is easily seen from table 3 or just by simple reasoning. This gives us six theorems altogether or 24 theorems if we multiply by the number of quadrants, and this is enough as a logical background for our inferences. The ambiguity in the last two rows of table 3 gives ut no difficulty, since we have assumed that S is true, and thus have assumed the least restricted case possible with the four truth-functions.

We now clearly see the implications of lack of reliability and inter-subjectivity:

1. Only if the instrument is *reliable* can we infer from measurement constancy to object-constancy *for the same tester*. If the instrument is *unreliable*, then measurement-constancy implies object-inconstancy, and measurement-inconstancy (which is the most likely case) implies object-constancy *or* inconstancy because of

    $$\vdash \text{S} \qquad\qquad \vdash \text{S}$$
    $$\vdash \text{M}_{s,s} \qquad\quad \vdash \sim \text{M}_{s,s}$$
    $$\vdash \underline{\sim \text{I}_{s,s}} \qquad\quad \vdash \underline{\sim \text{I}_{s,s}}$$
    $$\vdash \sim \text{O}_{s,s} \qquad \vdash \sim \text{O}_{s,s} \text{ or } \sim \text{O}_{s,s} \text{ which is a tautology.}$$

2. Only if the instrument is *intersubjective* can we infer from measurement-constancy to object-constancy *for different testers*. If the instrument is *not intersubjective,* then measurement-constancy implies object-constancy, and measurement-inconstancy (which is the most likely case) implies object-constancy *or* inconstancy, which means no information.

Of course, no inferences can be made on the basis of *one* measurement alone if the instrument is not known to be reliable or intersubjective —

this is like the case of testimony in court or in history, which does not (should not) increase in validity simply because there is nothing present to contradict it.

Finally, let us link together the concepts that have to do with instrument-properties: reliability, intersubjectivity and congruency. If we assume that the object measured is constant, they are related as follows:

Table 4. Instrument properties

	Measurements:	
Instruments:	constant	inconstant
same	same T: *reliability* diff. T: *intersubjectivity*	same T: *unereliability* diff. T: *lack of intersubjectivity*
different	*congruency*	*not congruency*

4. Necessary and sufficient empirical assumptions about instrument- and object-constancies

We turn now to the between-quadrant relations, and it is easily seen that they are of an empirical nature. No purely deductive procedure can lead us from short-time reliability to long-time reliability; from reliability to intersubjectivity or from one kind of object-constancy to the other kinds. The kind of assumptions we have to make should hence, at least in principle, be empirically testable. Let us look at two examples to illustrate the kind of reasoning made.

Length-measurement

If the object to be measured is such that we think it is constant when the same person measures it twice with a short interval in between, we would in most cases infer that it is constant for long intervals and for different persons, too. In short, we make the assumption:

$$\vdash \quad (i)(j)(k) \, [O_{s,s}(T_i) \gtrless O_{l,s}(T_i) \gtrless O_{s,d}(T_j,T_k) \gtrless O_{l,d}(T_j,T_k)] \quad j \neq k$$

This is only an equivalence-assumption (if one of them is true, then all are true, and if one is false, then all are false) but if we feed it with the assumptions (substantiated by data or by "intuition"):

$$\vdash \ (Ei)O_{s,s}(T_i) \rightarrow (i)O_{s,s} \ (T_i) \ \text{ and } \ \vdash \ (Ei)O_{s,s}(T_i)$$

then all of them become true, and we have an excellent basis for inferences concerning the properties of the instrument: short- and long-time reliability and intersubjectivity. The last two assumptions would read: .If the object is short-time constant for *one* person, then it is short-time constant for *all* persons in the competence-group and we assume, or the object is in fact, constant for one person.

The famous astronomical case with the tester's "personal equation" evidently has to do with short- and/or long-time intersubjectivity (the reliability is often assumed) (1, p. 52—53). In this case, the instrument (the telescope + the part of the tester engaged) apparently is not intersubjective, but a correction has been found so that the measurements of the various testers can be reduced to a fixed base, defined according to the measurements of a master-tester or the mean of a series of measurements (the mean is there often called the "truth"). This raises some interesting methodological questions we shall return to later. In astronomy, the assumptions above seem very plausible indeed.

Interviewing

In this case, we would be much less prone to commit ourselves to assumptions about the object's constancy. We know that some attitudes change rapidly, and that other attitudes are fairly constant. In some cases, therefore, we would make these assumptions:

$$\vdash \ (Ei)O_{s,s} \ (T_i) \ \gtrless \ (i)O_{s,s} \ (T_i)$$
$$\vdash \ (i)O_{s,s} \ (T_i) \ \rightarrow \ (i)O_{l,s} \ (T_i)$$

i.e., if the respondent has a constant attitude during a short-time interval for one tester (interviewer), then he would be constant for the other interviewers too (and conversely, of course) — and if he is so, then he would probably be constant for a long-time interval, too. The last conclusion would follow immediately if we have reasons for making the assumption

$$\vdash \ (Ei)O_{s,s} \ (T_i)$$

(that there really exists at least one tester for whom the respondent is constant). A similar chain can be established for different persons, from short-time constancy for one pair to all pairs, from short-time

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constancy for all pairs to long-time constancy for all pairs, and finally: the assumption that we have short-time constancy for one pair. This means simply that the object does not change attitude from one interviewer to the other, if the time interval between the two interviews is "short".

The last kind of assumption we can make would be from same-person-constancy to different-person constancy, and we know that this is often a risky assumption in interviewing, especially in informal interviews.

Let us then turn to the instrument side of the question. What assumptions can we reasonably make? In most cases, we assume that there are some interviewers *for whom* I is constant, at least over short intervals, and use them as standards for the other interviewers — all the time with the assumption that the object has the corresponding constancy. If we know nothing about the object we are forced to make assumptions of this kind, but they should be avoided.

However, we can often make assumptions similar to those made above. An important assumption seems to be:

$$\text{(i) } I_{s,s} (T_i) \rightarrow \text{(i) } I_{l,s} (T_i)$$

To use the sign for material implication is dubious, here as in so many other applications of symbolic logic. The relation should rather be explicated for our use as an ambiguous function S, defined by:

Table 5. The truth-function S

	$I_{s,s}$	$I_{l,s}$	$S (I_{s,s}, I_{l,s})$
1.	t	t	t
(2.)	t	f	f
3.	f	t	t,f
4.	f	f	t,f

i.e., a relation between short-time reliability and long-time reliability that permits us to infer from the one to the other according to this scheme:

$$\vdash S$$
$$\vdash I_{s,s}$$
$$\overline{\therefore \vdash I_{l,s}}$$

As this is a *modus ponens* inference, we shall retain the implication-arrow as a symbol.

As a rule the first of these premises will be less of a risk to make than the second. It is often reasonable to assume, as we do when we assert S, that *if* an interviewer has attained a sufficient proficiency to handle his instrument in such a way that the instrument is short-time constant *for him,* and he does not change his "style" or is without training for a longer period, *then* he should have a long-time reliable instrument, too (the converse need not be true, as we can see by inspecting Table 5). But to assume that an interviewer really handles his interview guide or what it might be in a short-time constant way is more than dubious. The best thing is apparently to assume object-constancy over short time for the same person(s), and find out the rest from an inspection of the measurements.

The remarks above can easily be extended to intersubjectivity.

Other examples could be added to the list. In the case of a medical diagnosis, for instance, the assumption is very often that it is possible to select a part of the patient's *soma* and apply short- and long-time reliable and intersubjective instruments to it — so-called "objective diagnosis", by means of chemical tests, counters, etc. The object's constancy or lack of it can then be deduced from the measurements. In the case of a judge passing sentences, however, or an instructor grading exams, all assumptions about instrument- and object-constancy (except perhaps $\vdash (Ei)O_{s,s}(T_i)$) seem dubious.

A technique often made use of when the constancy of an object shall be investigated with an instrument whose reliability and inter-subjectivity are not well known, is to test it out on some objects whose constancy one does not doubt. For the calibration of a thermometer, for instance, melting and boiling points of the same known compounds are used several times to check the properties of the instrument, and these are in turn made use of to make inferences about unknown compounds. The difficulty is, of course, to make sure that there is nothing different in the objects that is a relevant condition for the proper functioning of the instrument.

Instead of presenting a complete list of the rather trivial rules we can deduce from our analysis and the possible assumptions we can make, let us take one example from actual research. We are concerned with a test of the tensions in various muscles in the human body, where the instrument can be described as "inspection, mainly by palpation" (6). As muscle-tensions seem to have considerable diagnostic and predictive value, instrument- and object-constancies in all four quadrants are of interest. To obtain any basis for inference,

comparable series of measurements must be made, and we assume that we have decided on a rule so as to decide whether the measurements are constant or not, in the four quadrants. But these four items of information are not sufficient for inferences about eight constancies.

The problem then becomes: which minimum assumptions can we reasonably make that enable us to make these inferences. We have here a very complex case, where it is empirically impossible to say exactly where T ends and I begins, and it may even be difficult to say where I ends and O begins.

There seem to be two basic kinds of assumptions we can make; *unqualified* assumptions where we postulate on a more or less sound empirical basis that one particular constancy is factually true, and *qualified* assumptions where we make the truth of one constancy dependent on the truth of one or more others, e.g., if there is short-time intersubjectivity, then there is long-time intersubjectivity too. The qualified assumptions can be of a higher order involving more propositions than two only, as when we postulate: "short-time intersubjectivity *and* long-time reliability together imply long-time intersubjectivity for the same testers", or in symbols:

$$\vdash \quad (j)(k) \quad [I_{s,d}(T_j,T_k) \; \& \; I_{l,s}(T_j) \; \& \; I_{l,s}(T_k) \quad \rightarrow \quad I_{l,d}(T_j,T_k)]$$

which seems to be a reasonable assumption to make in more formal types of interviewing research.

In our example dealing with muscle-tensions the assumptions must be very weak, which in practice means that we must have as few *unqualified* assumptions as possible. However, the minimum number is *one;* we must have one unqualified assumption to "feed into" all our qualified ones. The only thing we reasonably can assume is $O_{s,s}$ — that the object is short-time constant when exposed to the same tester, and by "short time" we mean maximum a few days. Further, we feel that it is safer to infer "horizontally" than "vertically" in the table when we are dealing with the object-constancies, i.e., from same-person constancy to different-person constancy, whereas long-term object constancy is a highly important empirical finding if it exists, and cannot possibly be included among the assumptions. But some kind of vertical inference must be made in order to complete the table, and we put the assumption on the instrument side and for the same tester —, i.e., we assume that short-time reliability implies long-time reliability. Higher order qualified assumptions were considered too dubious.

Our four assumptions thus become:

4.1 ⊢ (i)$O_{s,s}(T_i)$ *short-time constancy, same T.*
4.2 ⊢ (i)(j)(k) [$O_{s,s}(T_i) \rightarrow O_{s,d}(T_j,T_k)$]   j ≠ k
4.3 ⊢ (i)(j)(k) [$O_{l,s}(T_i) \rightarrow O_{l,d}(T_j,T_k)$]   j ≠ k
4.4 ⊢ (i) [$I_{s,s}(T_i) \rightarrow I_{l,s}(T_i)$]

Further, we assume that our data warrant the assumptions:

4.5 ⊢ (i)$M_{s,s}(T_i)$
4.6 ⊢ (i)$M_{l,s}(T_i)$
4.7 ⊢ (j)(k)$M_{s,d}(T_j,T_k)$ j ≠ k
4.8 ⊢ (j)(k)$M_{l,d}(T_j,T_k)$ j ≠ k

The inferences are now very easy to make. 4.1 and 4.5 give:

4.9 ⊢ (i)$I_{s,s}(T_i)$ *short-time reliability.*

4.4 and 4.9 give:

4.10 ⊢ (i)$I_{l,s}(T_i)$ *long-time reliability.*

We must now bring in the "horizontal" assumptions. 4.1 and 4.2 give:

4.11 ⊢ (j)(k)$O_{s,d}(T_j,T_k)$ j ≠ k   *short-time constancy, different T.*

4.11 and 4.7 give:

4.12 ⊢ (j)(k)$I_{s,d}(T_j,T_k)$ j ≠ k   *short-time intersubjectivity.*

4.10 and 4.6 give:

4.13 ⊢ (i)$O_{l,s}(T_i)$ *long-time constancy, same T.*

4.13 and 4.3 give:

4.14 ⊢ (j)(k)$O_{l,d}(T_j,T_k)$ j ≠ k   *long-time constancy, different T.*

Finally, 4.14 and 4.8 give:

4.15 ⊢ (j)(k)$I_{l,d}(T_j,T_k)$ j ≠ k   *long-time intersubjectivity.*

We let this suffice as an indication that our four assumptions are sufficient as a basis for the inference to all eight instrument- and object-

constancies. At the same time, we see that we cannot dispense with any of the assumptions, *if we* want all the results we have got, so they are also necessary. However, we can do without assumption 4.3 if we are not particularly interested in quadrant 4 results.

Of course, the result indicated is too perfect to be true. Dealing with empirical data, it is not very probable that 4.5 to 4.8 will hold for all the testers. They may hold for some or only for one — in which cases the all-quantifier must be placed by an existential quantifier — or for no tester at all, in which case we must put a negation sign in front of the M. However, we can reason with the existential quantifier just in the same way, provided that we know all the time *which* testers we refer to and that we refer to the *same* testers (i.e., we proceed with the subset of testers for whom all of the assumptions 4.5 to 4.8 are valid). We must, however, justify our procedure by defining the testers for whom 4.5 to 4.8 do not obtain as "novices", "erratic", etc., and have intersubjectively valid data for our judgments. Judgments of this kind will gain enormously in convincing power if they are made prior to the experiment, not *post hoc* only.

We have indicated earlier how we can proceed in our analysis just as well with negations as without — the result will only be negations in the appropriate places in 4.9 to 4.15.

In conclusion, let us point out the importance of a strategically optimal location of the assumptions one *has to* make if one wants to do research in reliability, intersubjectivity, and/or constancy. In our case, the logical and empirical assumptions were as indicated:

Table 6. The assumptions in the muscle-test design

	same tester			different testers		
short-time	$I_{s,s}$	$O_{s,s}$	$M_{s,s}$	$I_{s,d}$	$O_{s,d}$	$M_{s,d}$
long-time	$I_{l,s}$	$O_{l,s}$	$M_{l,s}$	$I_{l,d}$	$O_{l,d}$	$M_{l,d}$

The general problem is to build a bridge of empirically warranted assumptions sufficient to reach all quadrants. If we can assert the validity of the I or the O statement in each quadrant, then we shall have a sufficient basis, but this is not necessary. If we assert only one, then we shall need three "implication-bridges" (two horizontal, and one vertical, as above, or two vertical and one horizontal. We assume that diagonal

assumptions are too audacious). If we assert two, two implications will do; with three, one implication will do — in short, the sum of unqualified and qualified assumptions shall equal 4.

5. Summary

In this paper, we have given non-operational definitions of "reliability", "intersubjectivity" and "constancy"; the first two referring to instrument-constancies, and the third referring to empirical objects. It is shown how conclusions can be reached about these important properties of objects and instruments once we know whether the measurements made can be said to be constant or not. The relation between object-, instrument- and measurement constancy is spelled out as a set of closely related truth-functions to facilitate inferences about one of the three when we have (factual or postulated) knowledge about the two others. More specifically, it is shown with a concrete example how one can locate necessary and sufficient assumptions about the relations between different kinds of object and instrument constancy (for same tester or different testers, over short intervals and long intervals), so as to assume as little as possible, or at least not more than necessary. It is argued all through the paper that this is a kind of reasoning that often should be made more explicit in social science research.

NOTES

1. Carlsson, N. Gösta: *Dimensions of Behaviour,* Gleerup, Lund, 1949.
2. Ekman, Gösta: *Reliabilitet orb konstans,* Almqvist & Wiksell, Uppsala 1947.
3. Guilford, J. P.: *Psychometric Methods,* 2nd ed., McGraw-Hill, 1954.
4. Lazarsfeld, P. F.: *Latent Structure Analysis,* mimeo, 1957.
5. Newcomb, T.: *Social Psychology,* Tavistock, London, 1952.
6. Waal, Nic.: *Nic. Waals metode til somatisk psykodiagnostikk,* Oslo, 1957.
7. Zetterberg, Hans: *On Theory and Verification in Sociology,* Almqvist & Wiksell, Stockholm, 1954.

THE ALCHEMISTS OF SOCIOLOGY[1]

The eagle never lost so much time as when he submitted to learn of the crow.

WILLIAM BLAKE, *Proverbs of Hell*

The pretentiousness and verbiage which in some measure mar the social and psychological sciences are at last identified, and Professor Sorokin castigates them with much wit. He also attempts a diagnosis, and recommends a cure. I should like to record my disagreement with this final, constructive part of the book. The interest and merit of the book in no way depend on it; yet it is however only fair to record the fact that Sorokin attempts to be constructive in his criticism. Sorokin's diagnosis of the ills he so brilliantly describes is (p. 279) that they "are largely due to their faulty philosophy and theory of cognition." What is true is that, for the first time in history perhaps, we are faced with a (pseudo) empiricist scholasticism. By scholasticism, generically speaking, I mean an active but barren tradition whose barrenness is somehow ensured by its organisation, ideology and practices. What is novel here is an official adherence to the slogans, images and even practices of empiricism. But these practices are a kind of ritual, the performance of magical incantations and movements. Such a methodological ritualism, with its initiations, arcana and the whole works, should not be confused with empiricism.

It is true that what underlies this methodological ritualism is a kind of self-consciousness, which manifests itself in thinking more about method than about subject-matter. Preoccupation with method can be a kind of evasion of reality. "Method" calls forth a strange world of its own, separated from both the inner world of ideas and outer social reality. Indeed this alienation appears to be its function. The inner life is then atrophied and/or handed over to the psychiatrist, and outer reality ignored as un-amenable to true science. Method is conceived less as a

tool of discovery, than as a kind of *ersatz* tabula rasa, or as a means of approximating towards that state of purity . . .

Instead of what he considers the empiricist fallacy, Sorokin commends a new approach both to cognition and to the social reality cognised. But if in his constructive views Sorokin takes one into a somewhat slapdash and extravagant realm, that is only a small part of the book; in its major part, devoted to the Fads and Foibles, his touch is sure, his aim accurate and deadly.

What then, if not its positive contribution, makes this a significant book? In brief, it accurately portrays an important subculture within psycho-social science. These are no random fads and foibles; they clearly have common social and ideological roots. It would be difficult, and presumptious, for anyone who is not a careful student of the American sociological scene, to assess how widespread the participation in them is. But without presuming to make such an assessment,[2] and stressing that there are many untainted by it, and that many of those who are also produce distinguished and valuable work and are men of the highest ability, a casual observer is nevertheless in the position to confirm with conviction that such a subculture exists, and that Sorokin has hit it off.

All one can add is that if some of us are freer of these fads and foibles, it isn't for any lack of trying; we don't often have the opportunities. And there is a real glamour in the genuine vitality and *drang* associated with this Scientism.

But having made, repeated and stressed the qualification that this is only a portrait of a subculture within the larger world of American psycho-social science, the fact remains that it exists, — filled with feverishly active men with very curious horizons, lacking any past, history, or philosophy; within those low horizons of a few decades, a curious Daliesque world with a few simple kinds of things: some spindly statistical concepts, rickety structures of struts rising high upwards: a few psycho-analytic cumulus-cloud-concepts in the sky, and the rest a flat, flat plain strewn with a mass of indistinguishable and disconnected 'empirical studies'. Plus some indigenous 'theoretical conceptual frameworks', like the old isolated cupboards with half-open and empty drawers, standing alone, unconnected with the strewn objects or with the spindly structures which cast no shadow. In this strange world, lit with a cold light, a population of over-active and over-anxious eager men, who, no wonder, can only bear it by having recourse to those shamans of the Western world, the psycho-analysts. (Symbolised

perhaps in the picture by a huge all-seeing eye attached to nothing in particular.)

This world exists, curiously hygienic in its lack of historical and philosophical vistas. Its inhabitants, though anxious, are extremely confident and yet curiously touchy; often equipped with psychic deafness and a methodology instead of a mind. Between the aridity of statistical ritualism and the tropical lushness of psycho-analytic imagery, they wend their way to scientific truth, something like this: "Night follows day". Footnote: Cf. McAllister and Finkelstein, *Society in its Temporal Setting*. Or rather, "The functional sequentialization of diurnality by nocturnality has been established for a wide range of social structures." (I can't really do this kind of thing properly.) The best footnotes, too, are at the end, just as in real science, simply: McAllister and Finkelstein, 1952 *b*. Whatever else the psycho-analysts have not achieved, they have exorcised the logical super-egos of their wards.[3]

It simply won't do, as some very distinguished visitors have tried, to defend this kind of stuff by suggesting that those who object to it are wedded to a dilettantish, belle-lettristic conception of social study and fail to allow technical expression for new truths. For one thing, there are no new truths here, and the expression is not more but less accurate: these neologisms are generally a case of *l'art pour l'art*. It is not an effete aestheticism which makes one protest: the aesthetic criteria applied to expressions of thought are *not* independent of the logical criteria applicable to the *content*.

In his chapter on 'Obtuse Jargon and Sham-Scientific Slang' Sorokin refers to Schopenhauer's views on obtuse and foggy speech. Indeed, Schopenhouer's classic abuse of Hegel will ever remain the model for those who fight academic pretentiousness. But let us be fair to poor Hegel: whatever his defects, by being turned up-side-down he gave rise to one of the most important sociological theories. So much cannot be said for the contemporary version: a ball of fluffy cotton-wool looks exactly the same *any* way up.

There is, however, if Spinozists will forgive me, a certain similarity between it and Spinozism. Spinoza was awed by the very ideas of the possibility of rational explanation and of the unity of the world and the interdependence of things within it. He confused the assertion of these formal possibilities and the elaboration of related formal concepts with substantive theory. Some modern sociological theory confuses the assertion of the possibility of sociology, the elaboration or repetition of its most general concepts, and the idea of unity of and interaction

in society, with a genuine sociology. Spinoza was described as a *Gott-betrunkener Mensch,* and these theorists may equally merit the description of *Gesellschaft-betrunkene Menschen.* But: Spinoza had a good head for abstract concepts, and his intoxication led to no relaxation or logical control, no unrestrained release of conceptual *id.*

Those who doubt the existence of such a world must read Professor Sorokin's book. That is its merit. Its demerit, or rather its incompleteness, curiously similar to that of Krushchev's speech to the XX Congress,[4] is that it leaves unanswered the question of how the critic or anyone else could remain untainted, or indeed unabsorbed by that world; and it fails (despite illuminating obiter dicta) to provide a *social* explanation for the ills castigated: and this *is* required, for an erroneous epistemology, like the wickedness of one man, is not a sufficient explanation for ills so widespread and so institutionalised. If the indictment contained in Professor Sorokin's book were only partly true, how could he or anyone else escape the consequences of living in a world where such things go on? I do *not* mean that he too may be guilty of some of the fads and foibles: in fact, he rather nobly pleads guilty to this in the preface. It is not for me to say whether indeed he is, but it seems to me a small matter. All or most of us in the academic profession are guilty of some humbug some of the time. If only the innocent could cast stones at humbug few stones would ever be cast (the present reviewer would have to retire at once) and there would be little hope of progress. No: the issue is not individual innocence, but the very possibility of an individual escaping an atmosphere which seems so diffused and so well rooted. There is, after all, such a thing as Gresham's Law of Ideas. What happens to genuine ideas in a world in which so much that is vacuous has to be taken seriously? (One is puzzled by this just as one is by Krushchev's speech: one doesn't doubt that there were intelligent and honest men in Stalin's Russia, but one wonders how and to what extent they could remain so.) It is perhaps no accident that the most striking work on contemporary America has come from one who is not an academic social scientist at all, but a journalist: W. H. Whyte. And similarly, that when a really interesting work appeared, D. Riesman's, eyebrows were raised — we have the authority of the leading academic theoretician for this — amongst the pukka scientific sociologists . . .

One explanation which suggests itself is that we are in the presence of a *Cargo Cult.* The successes of the natural scientists have been so overwhelming, so manifest and so impressive that they can hardly be considered to be the same species with their colleagues. Non-scientific

subjects may indeed illustrate the wit, the elegance, the courage of the human spirit, but they also illustrate the Faustian awareness of the futility of the quest for knowledge. How many professors of non-scientific subjects — excluding men buried in the minutiae of their disciplines — do not sometimes feel like Faust's opening speech? ". . . und leider auch Theologie . . ." Let's be frank, *und leider auch* any other damn subject. But scientists are no longer Faustian men.

We know what may happen to cultures which are too suddenly brought into contact with the overwhelming wealth and technological superiority of another civilisation. They may develop a hysterical Cargo Cult, a desperate attempt at a short cut to the benefits of the superior technology. The pseudo-scientific features of the Fads and Foibles seem to me strictly analogous to the technological primitive who builds himself crude wooden imitations of western mechanical tools and then expects miraculous Cargo to arrive. The empiricism of American psycho-social science is sometimes attributed to the pragmatic temper of the American nation, but surely the striking feature of some of those pseudo-scientific gadgets is their practical irrelevance, cumbersomeness, indeed obstructiveness. This is even noticed by their users, but they don't mind: the miraculous Cargo will not arrive at once, one must have a little patience . . . The magic will work, but not just yet. No deadline is set for the demonstration of its efficacy. Now I am not saying that social and human studies cannot be "scientific" in the sense that there is no room for accuracy, neologism, system, quantification, any more than I doubt the capacity of Polynesians to learn Western techniques. But one can easily tell a Cargo cult from genuine adaption, by a certain hysteria, an uncomprehending concentration on the externals.

A different explanation which is sometimes put forward for the phenomena described by Sorokin is MacCarthyism. As an explanation this is wearing a bit thin now. It is however true that the tendency either to float in extremely abstract conceptual schemes,[5] or to bury one's nose in the sand of statistically camouflaged, very fact, does tend to have total innocuousness as its consequence — which surely ought to be a danger signal. *Wertfreiheit* is all very well, *Gedankenfreiheit* is too much. It is a strange thing when *sociological* statements become quite such milk-and-water.[6] The most quick-tempered policeman, in any regime you care to name, is unlikely to reach for his truncheon on reading them. I suspect that few if any of the theoretical books are even on the Index, and if they are not republished in the Soviet Union, it is not because they are dangerous.

Of course, it may be said that the near-triviality and innocuousness of the propositions discussed is deliberate and desirable.[7] Such appears to be the view of Professor Homans. No doubt it is true of Homans himself, clearly a man of genuinely impressive intellect, that when what he says is obvious, it is so because, rightly or wrongly, he subscribes to a methodological view which requires it. He has interesting ideas too, but, methodologically, he values them less, perhaps. But a very pretty picture emerges if we generalise this. Are indeed all the authors of those theoretical frameworks, etc. full of interesting ideas which they keep back in order to build the unexciting groundwork first? If so, one can only take off one's hat in humble admiration for their truly iron self-restraint.

There is another way, more specific than general innocuousness, in which the Fads and Foibles perhaps serve current social trends. The ideas and values implicit in the study of small groups may reinforce the conformist trends of the time; the general theory, with its image of roles, interaction, equilibrium etc., though in one sense tautological, also suggests a concrete picture, a society of status rather than contract, and may help provide a rationale and a favourable state of mind for an emerging social order. If so it would be curiously analogous to classical economic theory, which also sets out to be a universal descriptive theory, is also tautological in its stricter interpretation, and also in effect helped create some of the phenomena in its own image, nature imitating science as well as art.

One can only suspect and vaguely discern the social explanations. Psycho-social studies have boomed in the United States, presumably with the expansion of higher education. Lacking firm traditions, they have been easier prey for fads than other subjects, and needed them more to simulate achievement and attract attention and financial support. The expansion created a vacuum, intellectually and institutionally speaking, which had to be filled: sociological theory, like God, had to be invented if it did not exist. Those who invented it made more of a hash of it than did the prophets and priests who invented God, and they never attempted the *via negativa,* at least not knowingly. I suppose borderline sciences or disciplines can be divided into (a) *Misgeburten* and (b) test-tube babies: on the one hand nature's errors, subjects which logically have no right to exist, which no one would have put on an *a priori* list, but which have a curious vigour nonetheless and resist attempts at liquidation (philosophy, psycho-analysis, perhaps even social anthropology as an autonomous discipline.) On the other hand, scientifically

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planned and designed creations, which logically must exist but into which the breath of life has only entered feebly if at all ... Self-conscious sociological theory of the Action Theory kind, carefully designed from the best methodological recipes, appears to be amongst these.

The milieu and atmosphere in which fads and foibles come into being would require the skill of a Budd Schulberg to describe them. *What makes psychosocial science run?* For run it does, in a world of total anomie, especially intellectual. Some of the foibles can only thrive where no one is sure of his intellectual standards and violent and unbridled competitiveness combines with a sad conformism, a mania for massive production with lack of genuine cumulativeness and criticism. The big prizes available to the truly successful, the insecurity protracted late into academic life for the majority, the doubtful competence of the external adjudicators in the strife — all these perhaps, contribute.[8] These factors presumably help to explain why such genuinely great ability, such numbers, such unquestionable energy and hard work, not to mention the financial facilities, lead generally to such uninteresting results. Since the appearance of *Fads and Foibles*, some of the sociological problems inadequately pursued in it have been implicitly answered by *The Organisation Man*.

One of the questions arising from that book is whether we must choose between conformism and a "Protestant Ethic", (whose attractiveness and perhaps reality it retrospectively exaggerates.) University departments in the psycho-social sciences seem doubly hit: in terms of their internal competitiveness they are still in a nineteenth-century stage, tough enough for the requirements of the most stringent of Protestant Ethics, whilst at the same time the values and outlook of the bureaucrat have come to dominate the content and style of research. In addition to the pseudo-scientific ideology, it is the influence of the bureaucratic norms which is probably an important factor making for the thinness of results, the appalling verbiage, the deliberate prolixity and tedious presentation, and so on. There is, after all, a well-known sociological law about him who pays the piper calling the tune. The piper is generally paid by the Foundation.

Now if the well-known multi-millionare, Mr. Cheeseburger, were to take a fancy to social scientists and encourage and aid them, the chances are that he would end up by supporting quite a few phoneys. One thing, however, he would not support, and that is *dullness*, for Mr. Cheeseburger personally, like most people, doesn't like being bored.

But Mr. Cheeseburger has not taken a fancy to anyone. He created a Foundation so as to keep control of the Cheeseburger enterprises. The Cheeseburger *Foundation* is run by highly competent, responsible administrators. *They* do not mind dullness: on the contrary, if it is the right kind, they welcome it, insist on it. The essence of a good administrator is that he is accountable, and behaves as if he were, even if not called to account: he honours his trust. Having handed out some money, he wants to see results: he may not understand them, he is not expected to be a specialist in everything even if he was himself once in the academic field. Now what could be better evidence of work done, of money well spent, than the number of pages covered, the number of questionnaires filled up, the number of calculations made, the number of variables considered, etc. etc. The prose and style of work widely adopted is simply not explicable[9] without the fact that its standards have evolved with an eye on the Foundation bureaucrat, to the specifications of the middlemen of the intellect, who need *weight* to justify expense. The bureaucrat is almost ex officio debarred by his impartiality from assessing merits of rival ideas, but he can count pages.

The interesting and ironic thing is that this happens even when a Foundation is sophisticated and strives to go against the dangers it discerns in the current scene. One distinguished Foundation, with a good reputation, distributes grants to individual scholars and, according to its pamphlet, wishes the grants to be used for thought, orientation, reflection, and not for the completion of specific *projects,* etc. So far so good: but despite this being the official ideology of the thing, the Foundation sees no contradiction in placing the holders of such a grant under the supervision of advisors, whose advice comes at the same time as the money, the supply of which they control, and who (1) know little or nothing about the subject, (2) have no inhibitions about offering advice none the less, concerning for instance the precise limits of the general thinking that is to take place(!) and (3) giving weight to that advice by financial pressure. The only effects this kind of situation can have is either a cynical and superficial adaptation by the Thinker, or a partial acceptance of those criteria of academic work which seem so self-evident to the bureaucrat that he doesn't know he holds them; or a mixture of both.

Of course, the main consequence of the manner of financing research is the development of what may be called managerial, entrepreneurial research.[10] The managerial Professor is now as familiar as the managerial General. Here again, we are faced with something that seems to flow

inevitably from the social realities. A successful academic generally reaches the highest available rank, full professor, some time fairly early in middle age, with many energetic years yet ahead of him. Leaving aside academic administration, which involves him in abandoning his subject, what is open to him? Of course, he could content himself, as he often did in the past, with the informal rewards of an intellectual influence. But intellectual influence can only be exercised through ideas, and in the fads-and-foibles atmosphere ideas have little prestige: few would recognise an idea under the jargon if they met one, and if they did they would not care. (Are we not turning social studies into a *science*, and hence obliged to despise mere *speculation?*) This being so, there is one way conspicuously open: bigger and bigger *un*-speculation, large scale *research*. The money for it is of course not misappropriated by the successful research-entrepreneur. But it gives him patronage and hence power, and money actually owned would not be usable in any other way. We can all think of some of the great Czars of research, sometimes men of no particular intellectual distinction. A research empire, once established, has a vested interest in the research paraphernalia, in self-justifying quantity, and often in unintelligibility. The size of the research, the complexity of the tools, the abstruseness of the language, are a kind of Conspicuous Display which is essential to the prestige of the enterprise and its further expansion. They are also a protective smoke-screen. It can no more confess failure than can any other political organisation. It can hardly grope through trial and confessed error. Defeats are not admitted, and the premium is on the kind of question and technique and language which appears to give some answer anyway. The supervising bureaucrat is happy, he has something massive with which to balance the money spent, and the Czar is happy, for the Empire prospers. Their symbiosis is interesting. The sociology of *the gift* has been explored, but the equally fascinating sociology of *the grant*, with its profound implications for donor and recipient, awaits its investigator. Of course, there are many techniques which may be genuinely useful or essential, and there are many problems which can only be answered by organised large-scale research: the autonomous research-for-its sake, or for the sake of the Empire to which it gives birth, flourishes amongst and to the detriment of its genuine rivals.

The merit of Sorokin's book is that it establishes beyond doubt that there is a world of Fads and Foibles of some size, and not merely the product of a jaundiced observer's eye. It portrays the world of men

who think they are the Newtons or the Royal Society of psychological and social science, and who in fact are its alchemists. Sorokin's witty account of them will live as a historical document long after those works themselves are forgotten.

Ernest Gellner.

NOTES

[1] Pitrim A. Sorokin: Fads and Foibles in Modern Sociology and Related Sciences. Henry Regner Co., Chicago 1956. $ 10. (Mayflower Publishing Co., London, 1958. 50 s.) 357 pp.

[2] We are all looking forward to Professor Wright Mills' forthcoming book on this.

[3] I was once assured, in praise of psycho-analysis, that one of the leading sociological theoreticians used to be merely a scholar, of the old European kind, until he underwent analysis, after which he acquired wings and was freed of his shackles. This is undeniable.

[4] I owe this point to Mr. P. Stirling.

[5] "The repellent recipe" as Mrs. J. Floud has appositely called one of them.

[6] I own a respectable Introduction to Sociology, printed in 1948, officially used for teaching by a reputable University, whose competent chapter on the history of the subject manages to avoid mentioning the name of Marx.

[7] Cf. some very interesting comments on this by Mr. A. H. Halsey in "British Universities and Intellectual Life", *The Universities Quarterly*, February 1958.

[8] I am pleased to have my superficial impression confirmed by Mr. N. Birnbaum, in "America – a Partial View", *Commentary*, 1958.

[9] Cf. Mr. H. J. Gans' "Informal Sociology" in *American Sociological Review*, August 1958, pp. 441–2.

[10] Cf. Mr. B. Moore's letter in *The Times Literary Supplement*, 30. 10. 58.

ARISTOTLE AND THE AMBIGUITY OF AMBIGUITY

by

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1. *Two types of ambiguity.*[1] Many important arguments of Aristotle's turn on the notion of ambiguity, both when he is criticizing others and when he is doing his own philosophy. A celebrated instance of the latter is his defence of the study of 'being as being' (*i. e.* metaphysics) in *Metaphysica* IV, 2:

> There are many senses in which a thing may be said to 'be' (Τὸ δὲ ὂν λέγεται μὲν πολλαχῶς . . .), but all that 'is' is related to one central point (πρὸς ἕν), one definite kind of thing (μίαν τινὰ φύσιν), and is not said to 'be' by a mere ambiguity (. . . καὶ οὐχ ὁμωνύμως . . .). Everything which is healthy is related to health, one thing in the sense that it preserves health, another in the sense that it produces it, another in the sense that it is a symptom of health, another because it is capable of it. . . . As, then, there is one science which deals with all healthy things, the same applies in other cases also. . . . It is clear then that it is the work of one science to study the things that are, *qua* being. (Sir David Ross in the Oxford translation of the works of Aristotle, *Met.* IV, 2.)

These passages show at the same time that the Aristotelian notion of ambiguity is not as unambiguous as one perhaps hopes. We see Aristotle distinguishing two different kinds of multiplicity of meaning: on one hand what I shall call the *multiplicity of applications* (expressed by λέγεται πολλαχῶς) and on the other hand what Sir David calls 'mere ambiguity' but what I shall call *homonymy*. The latter term is chosen because it is already used by Aristotle. My usage will violate the current distinction between homonymy and ambiguity, according to which "homonyms are formally indistinguishable present descendants of dissimilar older forms, while an ambiguous word is single in origin and multiple only in usage". However, the violation is smaller than first appears. As observed by Professor W. V. Quine (to whose lectures

the last quotation is due), the only thing that distinguishes ambiguity and homonymy (in the customary sense of the word) is etymology, from which we may here simply look away.

In view of the importance of the notion of ambiguity for Aristotle's thought, it is worth while to examine the varieties of this notion in some detail. In this paper, I shall try to show that Aristotle consistently distinguishes homonymy from multiplicity of applications and that he uses the terms ὁμώνυμος and πολλαχῶς for this purpose. I shall also argue that a number of other idioms are used by Aristotle to express multiplicity of applications as distinguished from homonymy. Furthermore, I shall offer a few comments on the nature of the distinction. In a second paper the results of this study will be used to analyze the varieties of the Stagirite's notion of possibility.

2. *Multiplicity of applications wider than homonymy.* There are many passages which show explicitly that for Aristotle multiplicity of applications is a broader notion than homonymy. The following passages are cases in point:

> We have pointed out elsewhere that 'potency' and the word 'can' have several senses (λέγεται πολλαχῶς). Of these we may neglect all the potencies that are so called by homonymy. (*Met.* IX, 1, 1046ª4 — 7.)

> Since the science of the philosopher treats of being *qua* being universally and not in respect of a part of it, and 'being' has many senses and is not used in one only (τὸ δ' ὂν πολλαχῶς καὶ οὐ καθ' ἕνα λέγεται τρόπον), it follows that if the word is used homonymously and in virtue of nothing in common to its various uses, being does not fall under one science (for the meanings of a homonymous term do not form one genus); but if the word is used in virtue of something in common (κατά τι κοινόν), being will fall under one science. The term seems to be used in the way we have mentioned, like 'medical' and 'healthy'. For each of these also we use in many senses (πολλαχῶς λέγομεν). (*Met.* XI, 3, 1060ᵇ31 ff.)

> Now every term which possesses a variety of meanings (λέγεται πολλαχῶς) includes those various meanings either owing to a mere coincidence of language (ὁμωνύμως), or owing to a real order of derivation in the different things to which it is applied. (*De Gen. et Corr.* I, 6, 322ᵇ29 ff.)[2]

The following passage clearly also pertains to the same distinction, although the term πολλαχῶς does not occur:

> ... essence will belong, just as 'what a thing is', primarily and in the simple sense to substance, and in a secondary way to the other categories also, ... For it must be either by a homonymy that we say these *are* (in the way in which that which is not known may be said to

be known), the truth being that we use the word neither homonymously nor in the same sense, but just as we apply the word 'medical' by virtue of a *reference* to one and the same thing (πρὸς τὸ αὐτὸ μὲν καὶ ἕν), not *meaning* one and the same thing, nor yet homonymously; for a patient and an operation and an instrument are called medical neither by homonymy nor with a single meaning (καθ' ἕν), but with reference to a common end (πρὸς ἕν). (*Met.* VII, 4, 1030ᵃ29 ff.)

3. *Homonymy equals accidental homonymy.* These quotations (and all the others in the sequel) have been adapted from the Oxford translation of the works of Aristotle, the only changes being those occasioned by my use of the terms 'homonymous' and 'homonymy'. We shall have occasion to return to these passages later. Here I shall only note that in all of them τὰ πολλαχῶς λεγόμενα appears as a class wider than τὰ ὁμώνυμα. This is supported by many other passages. *(Cf. infra.)* And there is not, to the best of my knowledge, a single good counterinstance in the Aristotelian *Corpus*. A few passages may seem to suggest a different relation between τὰ ὁμώνυμα and τὰ πολλαχῶς λεγόμενα. But on closer examination they turn out to be compatible with what has been said here. A case in point is *Eth. Nic.* I, 6, 1096ᵇ27 ff. where Aristotle refers to as 'accidental homonyms' (τὰ ἀπὸ τύχης ὁμώνυμα) terms which he elsewhere calls homonyms *simpliciter.* Does this mean that Aristotle has two senses of homonymy, a wider and a narrower? Most likely it is only an indication of the origin of Aristotle's terminology. Literally, τὰ ὁμώνυμα are things which share the same name; and this seems to have been the common usage before Aristotle. Used in this way, the term does not say anything about whether or not the things share anything more than the name. In fact, τὰ συνώνυμα was generally used as a synonym of τὰ ὁμώνυμα. Aristotle himself occasionally uses ὁμωνυμία in this sense; witness *De Gen. et Corr.* I, 10, 328ᵇ21; *Met.* I, 9, 990ᵇ6; *Met.* VII, 9, 1034ᵃ22, 23, ᵇ1; *Met.* XIII, 4, 1079ᵃ2 and *Met.* XIII, 10, 1086ᵇ27. (*Phys.* VII, 3, 245ᵇ16, *De Part. An.* I, 3, 643ᵇ7, and II, 2, 647ᵇ18 may also be cases in point.)

In his more technical terminology, however, Aristotle distinguished between homonyms and synonyms. Both the homonyms and the synonyms share a term (ὄνομα). However, homonyms share it completely as a matter of chance; they share it, but do not share anything else. In short, by τὰ ὁμώνυμα Aristotle *meant* τὰ ἀπὸ τύχης ὁμώνυμα. This explains why the latter expression is used for the former in *Eth. Nic.* I, 6, 1096ᵇ27. It also explains the other passages in which Aristotle refers to as accidental homonymy what obviously is his usual notion of homonymy. (See *Eth. Eud.* VII, 2, 1236ᵃ17 and ᵇ25.)[3]

4. *An explanation of Aristotle's choice of terms.* A comparison between the origins of the terms ὁμώνυμος and πολλαχῶς shows why it is idiomatically natural that the former should express a stronger notion than the latter. ὁμωνυμία suggests to Aristotle that two things share a name, an ὄνομα, and nothing else, that the 'name' has a completely different meaning in the two applications. In contrast, πολλαχῶς ordinarily means 'in many ways'. (πολλαχῶς or one of the subordinate terms διχῶς and τριχῶς is occasionally used by Aristotle in this way without any reference to ambiguity; see *e.g. An. Pr.* I, 23, 41ᵃ14, and 26, 42ᵇ36-41; *Top.* III, 6, 120ᵃ15-25, and IV, 3, 124ᵃ3; *Phys.* VIII, 1, 250ᵇ23; *De Caelo* II, 4, 287ᵇ22; *Meteor.* IV, 3, 380ᵇ4; as well as *Eth. Nic.* II, 6, 1106ᵇ29.) This makes the term very apt to express any diversity of applications, whether it be due to a real difference in meaning, to different relations to one and the same basic meaning, or just to a difference in context.

5. *Homonymy* v. *synonymy.* Aristotle explains his sense of homonymy (together with that of the contrary notion of synonymy) in the beginning of the *Categoriae.* According to these explanations, two things are synonyms if both the same 'name' (*i.e.* term) and the same definition (λόγος) is applicable to them. They are homonyms if they share only the name, the definitions (λόγοι) being different in the two cases. (In these definitions, λόγος should perhaps be understood as an explanatory phrase or as an account of the meaning of a name rather than as a definition.) I have already pointed out that Aristotle sometimes violates his definition of homonymy. Similarly, he violates the definition of synonymy at least once by calling a pair of objects synonyms which only share the name but not the definition (see *Met.* I, 6, 987ᵇ10; *cf. Met.* X, 10, 1059ᵃ13).

These violations are little more than occasional reversions to looser usage. But in another respect Aristotle violates the definitions of homonymy and synonymy given in *Cat.* 1 almost systematically. In so far as the definitions are concerned, only things can be called homonymous or synonymous, not words. And two things can be called synonymous only if the *same* term is applied to them. Both these limitations are transgressed by Aristotle. A word is said to be homonymous in *De Gen. et Corr.* I, 6, 322ᵇ29 (*cf.* also *Top.* V, 2, 129ᵇ30 ff.); and similar uses of the notion of synonymy are found in *Top.* VIII, 13, 162ᵇ38, *De Soph. El.* 5, 167ᵃ24 and in *Rhet.* III, 2, 1404ᵇ37-1405ᵃ2. In many other passages, too, Aristotle is obviously interested exclusively in the word and not in the things to which it is applied. In fact, he often expresses synonymy and homonymy by such expressions as ἓν σημαίνειν and πολλὰ σημαίνειν (or πλείω σημαίνειν), respectively. In the sequel, we shall take the same liberty as Aristotle and talk about synonymy

(homonymy) both in connection with certain terms and with the entities to which they are applied.

6. *Homonymy* v. *synonymy not really a dichotomy*. Observing that the explanations given in *Cat*. 1 are not entirely representative of Aristotle's usage helps us to dispose of a difficulty to which these explanations give rise. I am arguing that Aristotle has two notions of ambiguity, of which that expressed by πολλαχῶς is wider than homonymy. From the explanations of *Cat*. 1 it may appear that there is no room for a notion wider than homonymy. Each pair of things which share a name either also share the definition or do not share it. Hence the distinction seems to constitute a dichotomy. However, Aristotle goes on to define a third notion, *viz*. that of paronymy. This notion has often been taken by commentators to be somehow intermediate between homonymy and synonymy. (See *e. g*. Simplicius, *In Aristotelis Categorias Commentarium*, ed. by C. Kalbfleisch, Berlin, 1907, p. 37, lines 3-4: "paronyms are somehow between homonyms and synonyms, sharing in both and being deficient of both".) But such an interpretation is not justified by the text of the *Categoriae* alone, for there paronymy appears as a notion incomparable with synonymy and homonymy: Things are defined to be synonyms or homonyms in so far as they share the *same* name, whereas two things are paronyms when they are called by different 'names' (terms) of which one is nevertheless derived (grammatically) from the other. However, this objection to conceiving of paronyms as an intermediate class can be discounted by observing that Aristotle himself frequently uses his notion of synonymy in connection with two different terms which share a definition (*i. e*. terms which are synonymous in the customary modern sense of the word). Cases in point are found in *Top*. VIII, 13, 162ᵇ38, *De Soph. El*. 5, 167ᵃ24, *Met*. IV, 4, 1006ᵇ18 and *Rhet*. III, 2, 1405ᵃ1 ff. It is perhaps significant that Aristotle does not often contrast the terms 'homonymous' and synonymous'. Most of these contrasts occur in the *Topica;* and even in the *Topica* synonymy is contrasted not only with homonymy but also with paronymy (*Top*. II, 2, 109ᵇ4 − 6).

7. *Partial and complete discrepancy of definitions*. The passages quoted in section 2 suggest a division which is more sophisticated than the explanations given in *Cat*. 1 and which is more representative of what Aristotle actually does. Broadly speaking, the view which emerges from the quotations seems to be this: The distinction between homonymy (complete difference of definitions) and synonymy (identity of definitions) is not a dichotomy because definitions may be partially identical, partially different. Those applications of a term in which the λόγοι are (completely) identical are synonymous (the expressions καθ' ἕν and ἓν σημαίνειν are also sometimes used by

Aristotle); those applications in which the definitions are partially or totally different are said to be πολλαχῶς λέγεται. On different occasions Aristotle gives different accounts of the class intermediate between synonyms and homonyms. Sometimes its members are said to have definitions which are different but still related to one central point (sc. to be πρὸς ἕν; cf. the quotation given in section 1); sometimes their definitions are said to be derivable from each other 'by adding to and taking away' (Met. VII, 4, 1030ª29, quoted in section 2). In Eth. Nic. I, 6, 1096ᵇ26 ff. the different non-homonymous senses of the term 'good' are said to be connected either by all being derivable from one kind of good (i.e. being ἀφ' ἑνὸς) or by all contributing to one kind of good or else by analogy. Sometimes the intermediate cases are simply said to have something in common. We also have paronymous terms which are derived from each other by some grammatical process. Obviously, then, there are several kinds of intermediate cases between synonymy and homonymy not all of which are listed by Aristotle in any particular passage. It does not seem possible to give a general description which would cover all of them.

8. *Instances of* πολλαχῶς. We can find many examples to illustrate these points and to confirm my claim that πολλαχῶς really covers all the intermediate cases. In Met. IV, 2, 1004ª22 ff. Aristotle is discussing the different opposites of 'one' (or the 'same'):

> ... since there are many senses (πολλαχῶς λέγεται) in which a thing may be said to be one, these terms also will have many senses, but yet it belongs to one science to know them all; for a term belongs to different sciences not if it has different senses (πολλαχῶς), but if it has different senses (sc. is not καθ' ἕν) *and* its definitions (λόγοι) cannot be referred to one central meaning (sc. are not πρὸς ἕν).

This criterion for all the different applications of a term to fall within one and the same science which is employed here is the one used by Aristotle in Met. XI, 3, 1060ᵇ31 ff. (quoted above in section 2). Again:

> ... all that is is said to 'be' in virtue of something single and common, though the term has many meanings (πολλαχῶς λεγόμενον) ... (Met. XI, 3, 1061ᵇ13 ff.)

The discussion in Met. IV, 4 is also relevant here. The following is a list of some passages where a construction with πολλαχῶς is used to indicate a multiplicity of applications different from homonymy; in them the different applications of the expression in question can be distinguished from each other by introducing a further qualification or can be derived in some other way from one and the same basic use: Phys. I, 7, 190ª31; Phys. V, 4, 227ᵇ3;

De Part. An. II, 2, 648ᵃ36, and 649ᵇ6; *Met.* IV, 2, 1005ᵃ7; *Met.* V, 11, 1019ᵃ5; *Met.* VII, 1, 1028ᵃ31; *Eth. Nic.* V, 9, 1136ᵇ29. We may also note that the contradictory of πολλαχῶς is sometimes expressed by Aristotle as ἁπλῶς (*e. g. Phys.* I, 3, 186ᵃ24; *cf. De Part. An.* II, 2, 648ᵇ11) — a word which by definition (*Top.* II, 11, 115ᵇ29 ff.) indicates the absence of qualifications. In *Met.* IV, 2 (*cf. supra*) the uses of 'being' and 'one' in the different categories are said to refer all to the same starting-point, thus showing that the different applications are not homonymous. For this reason, the following instances of πολλαχῶς stop short of homonymy: *Phys.* I, 2, 185ᵃ21, and ᵇ6; *Met.* I, 9, 992ᵇ19; *Met.* VII, 1, 1028ᵃ10; *Met.* XIV, 2, 1089ᵃ7, 16.

The obvious parity of πολλαχῶς with such expressions as πλεοναχῶς (and the interrogative ποσαχῶς) suggests that the latter ones are applicable in the same situations as the first one. Aristotle's usage shows that this expectation is justified. I have not found a trace of difference between the cases in which the three terms are applied, and often they are used obviously interchangeably.[4]

9. *A further difficulty.* This does not yet remove all our difficulties. The explanation of homonymy in terms of the λόγος does not always work. Often Aristotle indicates that the *logos*, the explanatory phrase used to express a thing's essence, may itself be ambiguous. (See *e. g. Top.* I, 15, 107ᵇ6 ff., *Top.* V, 2, 129ᵇ30—32, *Phys.* VII, 4, 248ᵇ17). The definitions given in *Cat.* 1 therefore do not take us very far. Now it might be suggested that for Aristotle every term had exactly one proper definition, and that the notion of homonymy is to be defined with respect to this unique definition proper rather than to any arbitrary accounts people may give of its meaning. This may very well be so. However, the explanations Aristotle gives as to how correct definitions are to be found do not help us here very much. Apparently they did not always help Aristotle himself very much, either. For instance, he is not consistent in classifying analogical and metaphorical uses of a word. In *Top.* VI, 2, 140ᵃ6—8 metaphorical expressions are distinguished from the homonymous, whereas in *Met.* IX, 1, 1046ᵃ6 certain metaphorical uses of a term turn out to be homonymous (by comparing this passage with *Met.* V, 12, 1019ᵇ33). In *Eth. Nic.* I, 6, 1096ᵇ27 ff. analogically connected things appear (as we have seen) as a subclass of τὰ πολλαχῶς λεγόμενα rather than of τὰ ὁμώνυμα. In *Met.* IX, 6, 1048ᵃ35 ff. it is said that 'actuality' and 'potentiality' are both analogical notions; yet Aristotle never worries about them in the way he is always wary of homonymy. In contrast, it appears from *Phys.* VII, 4, 249ᵃ23, 25 that analogical uses of a word may very well be homonymous.

The paramount fact is that some of Aristotle's favourite examples violate his own principles. It simply is not true that all the pairs of things he calls

homonyms have literally nothing in common except the term. For instance, Aristotle says repeatedly that a dead hand and a living hand, a dead eye and a living eye, *etc.* are homonyms (*Meteor.* IV, 12, 390ᵃ13; *De An.* II, 1, 412ᵇ22; *De Part. An.* I, 1, 640ᵇ36; *De Gen. An.* I, 19, 726ᵇ24; II, 1, 734ᵇ25; II, 1, 735ᵃ8; *Met.* VII, 10, 1035ᵇ25; *Pol.* I, 1, 1253ᵃ20—25). The reason why Aristotle says this is clear enough. He wants to define 'hand' or 'eye' in terms of the function of the organ in question. For this reason, the definition does not apply to a dead hand or a dead eye. But from this it does not follow that the definition of a dead hand or eye will have nothing in common with that of the living organ. It is difficult to see how it could avoid having part of the definition in common; a dead hand presumably has to be defined by reference to the living hand it used to be.

Similarly, in *Met.* I, 9, 991ᵃ5—8, Aristotle implies that a particular man and a statue (clearly one representing a man) can only be called 'men' by homonymy. Now it seems impossible to define a statue of a man without using the term 'man' or its definition in the process. Hence there ought to be something in common with the two applications of 'man'; and hence they ought to be classified as an instance of τὰ πολλαχῶς λεγόμενα rather than as an instance of τὰ ὁμώνυμα according to the letter of Aristotle's explanations.

The similarity of this example with Aristotle's own paradigm of homonymy (*Cat.* 1ᵃ2; ζῶον applied to a man and to a picture) suggests that even the latter should be classified among τὰ πολλαχῶς λεγόμενα if we were following Aristotle's instructions literally. It is possible, of course, that the Stagirite is here playing with the well-known ambiguity of the Greek word ζῶον. But this strikes me as a far too simple-minded point to be made by Aristotle, who had already in the Περὶ ἰδεῶν presented a highly sophisticated argument concerning essentially the same picture-paradigm situation.[5]

It is also difficult to believe that Aristotle should not have recognized any connection between the different applications of ξένος discussed in *Rhet.* III, 8, 1412ᵇ11 ff. One more instance of homonyms which certainly have something in common — if only by way of contrast — occurs in *Phys.* III, 3, 202ᵃ27—28.

10. *Terms with several meanings v. terms covering different cases.* We have to realize that the distinction between homonyms and terms which merely have different applications often amounts to something rather different from a distinction between complete and partial discrepancy of definitions. Sometimes it amounts to a difference between genuinely ambiguous terms on one hand and on the other hand terms whose fields of application fall into different parts for some non-logical reason. In particular, the applications of

a term are for Aristotle instances of τὰ πολλαχῶς λεγόμενα unless they fall
within one and the same species. This is shown by *Eth. Eud.* VII, 2, 1236ᵇ24
ff. where the contradictory of what is obviously πολλαχῶς λέγεται is re-
ferred to as "falling under one species". (*Cf.* also *Eth. Eud.* VII, 2, 1236ᵃ16
ff.) This point probably also underlies Aristotle's view that attributes which
are found within more than one species are defective in that they cannot be
a part of the essence of anything:

> Now specifically distinct animals cannot present in their essence a com-
> mon undifferentiated element, but any apparently common element
> must really be differentiated. (*De Part. An.* I, 3, 643ᵃ1 ff.)

Another way of putting essentially the same point is to say that Aristotle
is likely to speak of homonyms when he is stressing what we would call
a logical disparity of two applications of the same term, and of τὰ πολλαχῶς
λεγόμενα when he is dealing with some other kind of disparity of applications.
From this point of view, we can understand why Aristotle should call a dead
hand and a living hand homonyms; he was calling attention to what for him
was a *logical* difference between two kinds of application of the word 'hand'
Conversely, it is now understood why Aristotle uses the phrase πολλαχῶς
λέγεται or one of the subordinate locutions διχῶς λέγεται, τριχῶς λέγεται
etc. in many passages where he is not concerned with ambiguity of a term
at all in the modern sense of the word. (*Cf.* section 4 *supra* and for διχῶς
also section 13 *infra*.)

An especially clear-cut case in point is found in *An. Pr.* I, 17, 37ᵃ16.
Since the Stagirite is there in the midst of a logical discussion we may
reasonably expect him to be rather careful with his terms. He says that
the expression

(1)   it is not possible that no A is B

is used in two ways (διχῶς λέγεται); it may mean either

(2)   some A is necessarily B

or

(3)   some A is necessarily not B.

Why is this so? From Aristotle's definition of possibility (rather, of contin-
gency; see section 4 of my second paper) it follows that the unnegated part
of (1) or

(4)   it is possible that no A is B

is really a conjunction of two propositions, *viz.*

(5)   it is not necessary for any A to be B

and

(6)   it is not impossible for any A to be B.

The negation of (4), *i. e.* (1), consists therefore of two parts in the sense of
being a disjunction of two propositions. These propositions are, of course,
the negations of (5) and (6). But these negations are nothing but (2) and
(3), respectively. Hence Aristotle's point here is simply that (1) has two ap-
plications *in the sense of being the disjunction of the two propositions* (2) *and*
(3), either of which can make (1) true. Obviously, there is no semblance of
ambiguity here, in spite of Aristotle's use of the phrase διχῶς λέγεται.
Rather, what we have here is an unambiguous expression which covers two
different cases. Such an expression is comparable with a term whose field
of application falls into two parts for some reason different from ambiguity.

A similar instance of διχῶς occurs in *De Int.* 10, 19b20. Again, we find
no trace of ambiguity. Aristotle's point is that the copula 'is' may be used
to form two kinds of affirmative and negative propositions. As far as I can
see, it is not so much as suggested that any single sentence could have both
kinds of propositions as its meaning.

An objection suggests itself here. Many terms which we would never call
ambiguous cover more than one case in the sense of having a non-contin-
uous field of application. For instance, most of our general terms apply to
more than one individual. Is this not a very poor reason for trying to assim-
ilate them to ambiguous terms? Whether the reason is a poor one or not,
it is Aristotle's. It is, in effect, one of the reasons he gives (in *De Soph. El.* 1,
165a11 ff.) for studying sophistical refutations. These are due to the fact,
Aristotle says, that words and phrases often stand for more than one thing
(πλείω . . . σημαίνειν). And this is in turn inevitable, according to Aristotle,
because the number of words and phrases is finite while that of things is
infinite. His reason has therefore nothing to do with the meanings of terms;
it pertains solely to the multiplicity of things to which the terms apply.

11. *A guess at the sources of the distinction.* In fact, Aristotle sometimes
seems to assimilate terms whose field extends over more than one species
even to homonymous terms. There is a passage (*Phys.* V, 4, 228a24 ff.) which
suggests that for Aristotle the field of application of a non-homonymous
term must be continuous:

> Now some things have no extremities at all; and the extremities of
> others differ specifically and are homonymous; how should, e. g. the
> 'end' of a line and the 'end' of walking touch or come to be one?

This passage also suggests that terms which admit differences of species,
i. e. terms whose field of application falls into specifically different parts, are

homonymous. This suggestion is supported by *Phys.* VII, 4, 249ª3—8 and 21—25. The interpretation of *Phys.* VII, 4, is a very difficult matter; nevertheless I do not see how passages like the following can be understood without assuming that Aristotle thinks that specific differences within the field of application of a term make it homonymous:

> So we have now to consider how motion is differentiated; and this discussion serves to show that genus is not a unity but contains a plurality latent in it and distinct from it, and that in the case of homonymous terms sometimes the different senses are far removed from one another while sometimes there is a certain likeness between them, and sometimes again they are nearly related either generically or analogically, with the result that they seem not to be homonymous though they really are.

My interpretation is supported by such passages as the following (I shall modify the Oxford translation slightly):

> Evidently, then, there cannot be Forms such as some maintain, for then one man (*sc.* the sensible individual) would be perishable and another (*sc.* the ideal man) imperishable. Yet the Forms are said to be the same in species with the individuals and not merely to be homonymous with them; but things which differ in kind are farther apart than those which differ in species. (*Met.* X, 10, 1059ª10 ff.)

Here we can perhaps have a glimpse at the sources of Aristotle's distinction between homonyms and terms which merely have different applications. Classifying terms which merely have different cases with genuinely ambiguous terms easily leads to difficulties. For instance, it is obvious that the different things that are 'good' do not fall within any single species. Nevertheless, Aristotle held that they are all connected with each other. There seem to have been two incompatible tendencies influencing Aristotle's treatment of ambiguity. On one hand, he was suspicious of every division within the field of application of a term and tended to classify terms whose fields admitted of a difference in species with homonymous terms. On the other hand, Aristotle seems to have considered homonyms — one is tempted to say, puns — as fairly typical instances of ambiguity. However, such words as 'being', 'one', 'good', *etc.* readily give rise to conflicts between these tendencies. Another source of trouble is the doctrine of categories. In his early writings Aristotle clearly thought of terms which are used in more than one category as homonymous (see *e. g. Top.* I, 15, 107ª3—12). One reason for doing so is clear; if terms whose applications fall within different species are for this reason homonymous, the same holds *a fortiori* for terms whose applications fall into different categories. Yet Aristotle later came to think of some

such terms not as homonymous but as merely possessing a multiplicity of applications. He emphatically rejected the idea that such terms are mere homonyms, as witnessed by the passages quoted above in sections 1 and 2 from *Met*. IV, 2 and *Met*. VII, 4, respectively.

The intermediate class of non-homonymous τὰ πολλαχῶς λεγόμενα serves to cater for the difficult cases. It is to be expected that the terms which earlier were assimilated by Aristotle to homonyms merely because they covered different cases are now relegated to the terms 'said in many ways'. The passages quoted earlier in this section may therefore be taken to support my claim that in his most mature writings Aristotle classified terms with several cases as having different applications, *i. e.* as typical instances of τὰ πολλαχῶς λεγόμενα. It is probably not an accident that most of these passages are from works which are known to be relatively early (*Phys*. VII) or which betray an early preoccupation (criticism of Plato in *Met*. X, 10).

Gradually Aristotle seems to have attached more and more importance to the class of τὰ πολλαχῶς λεγόμενα, included more and more terms in it, and distinguished it more clearly from the homonyms. The primacy of substance over the other categories enabled Aristotle to connect the different uses of terms which are applied in several categories with each other; thus they too could be classified as τὰ πολλαχῶς λεγόμενα as distinguished from homonyms. Other types of systematically ambiguous terms followed suit.

12. *Homonymy in the* Topica. This view on the relation between τὰ πολλαχῶς λεγόμενα and τὰ ὁμώνυμα in Aristotle agrees with the fact that the two are not distinguished as sharply from each other in the *Topica* and in *De Sophisticis Elenchis* as in some other writings of Aristotle's. For it is generally agreed that these works (of which the second seems to be merely the final chapter of the first) are among the earliest in the Aristotelian *Corpus*. It must be pointed out, nevertheless, that even in *Top*. and in *De Soph. El*. we find a distinction between homonymy and multiplicity of applications. In *De Soph. El*., ὁμωνυμία occurs as one particular type of ambiguity, *viz*. as the ambiguity of a term. It is distinguished from ἀμφιβολία, *i. e.* from the ambiguity of a phrase (syntactic ambiguity), as well as from other fallacies which turn on the misuse of language. In contrast, πολλαχῶς λέγεται is used as a wider term which applies to all these fallacies. (See *De Soph. El*. 7, 169ᵃ22 ff.) Also, in *De Soph. El*. 6, 168ᵃ25 homonymy is represented as one of the many types of ambiguity that turn on double meaning (διττόν), while in *De Soph. El*. 19, 177ᵃ11—25 διττόν and πολλαχῶς are applied without perceptible difference.

In the *Topica*, the distinction between homonymy and multiplicity of applications is more blurred. But even there we find an unmistakable distinction.

In *Top*. II, 3, 110ᵇ16 ff. Aristotle discusses instances of τὰ πολλαχῶς λεγό-μενα which are explicitly said not to be cases of homonymy.

It may also be pointed out that in the *Topica* Aristotle had a good reason for disregarding the distinction between the different kinds of ambiguity. What he was interested in there was the technique of actual argumentation. Now in a disputation it may be quite as disastrous to fail to keep track of the different applications of a term as to fail to distinguish the different meanings of a homonymous term. This may have been one reason why Aristotle was not happy with terms which cover several different cases, and why he tended to treat them on a par with homonymous terms. In other words, the reason why Aristotle discusses the multiplicities of application of different words and phrases is not likely to be that they possess several irreconcilable meanings. Rather, he does it to gain as much clarity as pos-sible concerning the matter at hand:

> The reason why the term you use, or the whole expression signifying the property, should not bear more than one meaning is this, that an ex-pression bearing more than one meaning makes the object described obscure . . . (*Top*. V, 2, 129ᵇ35 ff.)

> It is useful to have examined the number of meanings of a term . . . for clearness' sake (for a man is more likely to know what it is he as-serts, if it has been made clear to him how many meanings it may have) . . . (*Top*. I, 18, 108ª18ff.)

It is characteristic of Aristotle that even when he realizes that a term may have several applications without any logical harm resulting therefrom, he still feels uncomfortable:

> We must first distinguish the senses in which we use the words 'un-generated' and 'generated', 'destructible' and 'indestructible'. These have many meanings (πολλαχῶς λεγομένων), and though it may make no difference to the argument, yet some confusion of mind must result from treating as uniform in its use a word which has several distinct applications (πολλαχῶς). (*De Caelo* I, 11, 280ᵇ1 ff.)

In the same vein, Aristotle says in *De Soph. El.* 17, 176ª4 ff. that one must not give a single answer to an ambiguous question even when the answer is the same no matter in what sense the question is taken. This may be contrasted with Aristotle's insistence in *An. Pr.* that one cannot ever obtain a false conclusion from true premises. In *De Soph. El.* Aristotle is therefore requiring much more than what is necessary for the purpose of obtaining true conclusions.

Because Aristotle's purpose in the *Topica* thus tends to confuse the dif-

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ference between homonymy and multiplicity of applications, one must try to avoid relying on this particular work of Aristotle's in elucidating the distinction. Similarly, the apparent earliness of this work ought to make one wary about generalizing from what one finds there.

13. *Further expressions for multiplicity of applications as distinguished from homonymy.* The only thing that remains to be done is to ascertain that Aristotle uses certain other terms in the same way as πολλαχῶς. I am not here interested in the whole range of terms employed by Aristotle; I shall confine my remarks to those terms which will figure in my second paper.

In many passages where Aristotle uses διχῶς, the different senses in question can be distinguished and sometimes are actually distinguished from each other by a further qualifying epithet. Thus, in *Phys.* I, 8, 191^b2, the phrase 'a doctor does something' is said to have two uses (διχῶς λέγεται), obviously because a doctor may do something *qua* a doctor or *qua* something else. In *Phys.* VI, 2, 233^a24, it is said that a continuum can be infinite in two ways (διχῶς λέγεται), either with respect to division or with respect to extremities. Similar uses of διχῶς λέγεται or of the plain διχῶς are to be found in *An. Post.* I, 2, 71^b33, *De Soph. El.* 18, 176^b32, *De Gen. et Corr.* I, 7, 324^a26 ff., *Met.* V, 23, 1023^a27, *Eth. Nic.* VII, 11, 1152^b27, *Pol.* I, 6, 1255^a4. We may note that in many of these cases Aristotle goes on using one and the same term although he has pointed out that it is used in different ways. Similarly, a construction with διχῶς is often used by Aristotle when an attribute has two applications in that a thing may have this attribute either actually or potentially; see *De Anima* III, 6, 430^b6, *Met.* IV, 5, 1009^a33, and *Eth. Nic.* VII, 3, 1146^b32. It is perfectly clear that such a duality of applications does not make the attribute itself ambiguous. In *Phys.* II, 2, 194^a12, 'nature' is said to be twofold, applying both to form and to matter. But it is very soon seen that the two uses of φύσις are not unrelated; in 194^a16 Aristotle speaks of 'two natures', and goes on to discuss whether the form or the matter of a thing is more properly its nature — something he could not do if 'nature' were homonymous.

I am not saying that the meaning of διχῶς has escaped the translators. Although the usage is not uniform, the Oxford translators frequently translate διχῶς and even δ. λέγεται without speaking of 'meaning' or 'ambiguity' at all. Cases in point are *De Int.* 10, 19^b21, *De Caelo* I, 11, 280^b13, *Met.* V, 4, 1014^a17, *Met.* VI, 4, 1029^b30, *Met.* XI, 9, 1065^b10, *Pol.* IV, 8, 1294^a7, and *Pol.* V, 1, 1301^b6.

These examples show that διχῶς is used by Aristotle in the same situations as πολλαχῶς; *i. e.* that it was not confined to cases of homonymy This appears also to be the case with the phrase κατὰ δύο τρόπους λέγεται.

It is used by Aristotle in *Met.* V, 8, 1017ᵇ23 to distinguish two uses of 'substance'. In *Met.* VIII, 1, 1042ᵃ29 it turns out that their difference is that of actuality and potentiality; hence we have a case of multiplicity of applications rather than of homonymy. In *Met.* V, 14, 1020ᵇ14, the phrase is used to distinguish two senses of 'quality', one of which is said to be more appropriate than the other. This comparability shows again that we are not dealing with homonymy. Further confirmation is found in the passage from *Met.* XI, 3, 1060ᵇ31 ff. which was quoted above in section 2. For if κατὰ δύο τρόπους λέγεται expresses duality of applications rather than homonymy, one is bound to expect that the phrase οὐ καθ' ἕνα λέγεται τρόπον which occurs in the passage in question will be tantamount to πολλαχῶς. This is obviously the case.

NOTES

1 Concerning Aristotle's treatment of ambiguity I have learned much from Mr. G. E. L. Owen of Corpus Christi College, Oxford. I hope that he will claim credit for whatever I may have borrowed from him. An early version of this paper was read by Professor Rogers Albritton and by Dr. Seth Benardete, to whose criticisms and comments I am also greatly indebted. The howlers are all mine.

2 I have followed Joachim's translation here. On the face of things at least, Aristotle's words seem to imply that *every* term is λέγεται πολλαχῶς. If this really is what Aristotle means, the passage in question is comparable with *De Soph. El.* 1, 165ᵃ11 ff., which I shall comment on at the end of section 10.

3 The synonymy of 'accidental homonymy' and plain 'homonymy' in Aristotle was already pointed out by Alexander; see *In Aristotelis Metaphysica Commentaria*, ed. M. Hayduck (Berlin, 1891), p. 241, line 26.

4 In *op. cit.* pp. 220—221 Simplicius also ascribes to Aristotle the view that πλεοναχῶς λέγεται applies to intermediate cases of ἀφ' ἑνός and πρὸς ἕν rather than to cases of homonymy or synonymy.

5 See *The Works of Aristotle,* translated under the editorship of Sir David Ross, vol. 12 (Select Fragments), pp. 127—128.

METHODOLOGICAL SUGGESTIONS
FROM A COMPARATIVE PSYCHOLOGY
OF KNOWLEDGE PROCESSES

by

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Introductory Abstract. Philosophers of science, in the course of making a sharp distinction between the tasks of the philosopher and those of the scientist, have pointed to the possibility of an empirical science of induction. A comparative psychology of knowledge processes is offered as one aspect of this potential enterprise. From fragments of such a psychology, methodological suggestions are drawn relevant to several chronic problems in the social sciences, including the publication of negative results from novel explorations, the operational diagnosis of dispositions, the status of aggregates of persons as social entities, and the validation of psychological tests.

INTRODUCTION

While an aspiration to justify induction in some logical, analytic manner is a continually recurrent one, the bulk of epistemologists and philosophers of science today join Hume in regarding this effort as impossible. Rather, not only are specific inductions matters of synthetic hypothesis of a scientific sort, but this also holds for general principles of efficacious inductive procedure, should any such be found. In the course of making this point Feigl (1956, pp. 25—26) has referred to the very kind of comparative science of induction with which this paper will attempt to deal. Discussing whether or not assumptions about the uniformity of nature are necessary to justify induction, he says: "I fail to see the philosophical importance of any attempt in this direction. If it were the success of *human adaptive learning* and *theorizing behavior* that is to be accounted for, I would be the first to admit that this is a

genuinely meaningful question — but surely a question of science, not of philosophy. This question can indeed be answered. And the answer is clearly along the lines of the *biology and psychology of knowledge.* It is the same sort of question that can be raised on a more lowly level in regard to the learning and generalizing behavior of that pet of our psychologists, the white rat. Given the rat's equipment of learning capacities, how complicated a maze will it be able to master, in how many trials, under what conditions of previous training, etc.? While it is a long way from the orientation of rats in a maze to the intellectual adaptations (if I may be forgiven the irreverent comparison) of the Newtons, Maxwells, and Einsteins in their theoretical constructions of the physical universe, the nature of the problem is the same." Earlier, in his important paper on "Existential Hypotheses: Realistic vs. Phenomenalistic Interpretations," he speaks of the pragmatic approach to scientific induction as being in itself an empirical science, "being the psychobio-sociology of cognitive behavior" (Feigl, 1950). Bergmann (1957, p. 51), discussing the difference between the philosopher's and the scientist's tasks, has said, "The logical analysis of science is one thing, the psychology of discovery is another thing. The former is a philosophical enterprise; the latter, if we only knew more about it, would be a branch of the science of psychology." While such statements have been made for the purpose of defining the task of the philosopher, they point in the process to a fascinating empirical enterprise.

What is the status of this "biology and psychology of knowledge" in which a rat's maze learning and a scientist's theory building would be compared? While it is obviously not an established discipline and while it has been pointed to as potential rather than actual, parts and propositions for it are springing up independently from a number of quarters. The most conspicuous and voluminous beginning is Piaget's genetic epistemology (e.g., Piaget, 1950; 1957; Mays, 1954; Beth, Mays, & Piaget 1957, Segall, 1957). Worth resurrecting is Baldwin's *Genetic Logic* (1906, 1908, 1911) in which the phrase "genetic epistemology" also appears. Ness's *Erkenntnis und wissenschaftliches Verhalten* (Ness, 1936) is relevant, as is indicated by its leitmotiv: "einer objektivpsykologischen Beschreibung des wissenschaftlichen Verhaltens." More recently, he has identified the theory of knowledge as a part of behavioral science, and the problem of "how is knowledge possible" with that of "how can rats learn" (Naess, 1948). Köhler's *The place of value in a world of facts* (1938) belongs in considerable part. Polya's (1945, 1954) interest in developing specific propositions for a "heuristic" is an

excellent example. Michotte's (1946) discussion of the relevance of his empirical research on the perception of causality to the writings of Hume, Kant, and Maine de Biran belongs here, as does Smythies' (1956) attempt to confront certain chronic epistemological problems with the evidence of modern neurology. The psychologist Craik's *The Nature of Explanation* (1943) is a stimulating and frustrating fragment. Bakan (1953) has called attention to the implicit theories of learning involved in the choices of scientific strategy on the part of learning theorists. When scientists have attempted to describe the process of science, their efforts have frequently been in this area. Take, for example, the works of Ritchie (1923), George (1936), Brown (1950), and Oppenheimer (1956). Lacking the distinction between a philosophy of science and a yet-to-be-developed science of science, such works have often been mistaken for amateur, and hence second-rate, philosophy of science.

As in any unconsolidated scientific discipline, there are bound to be a variety of approaches and hypotheses, not all in agreement. The present paper draws from the recent revival of interest in evolutionary theory: not so much in the fact of evolutionary sequences *per se,* as in the mechanism of progressive adaptation through blind variation and selective survival. The application of such a perspective to knowledge processes appears in von Bertalanffy's "An Essay on the Relativity of Categories" (1955), in Platt's "Amplification Aspects of Biological Response and Mental Activity" (1956), in Lorenz's "Die angeborenen Formen möglicher Erfahrung" (1943) and his "The Rule of Gestalt Perception in Animal and Human Behavior" (1951). The present writer was initially led to this explicit orientation through Ashby's *Design for a Brain* (1952) supplemented by Pringle's "On the Parallel between Evolution and Learning" (1951). In addition, the great influence of Brunswik and Tolman should be stressed. Brunswik, for example, was willing to regard unconscious perceptual processes as "ratiomorphic" (1957) and made free use of Darwinian considerations at a time when it was considered *passé* to do so. From Brunswik's (1952, 1956) presentation of perception, I have received inspiration for an orientation toward knowledge processes in general. Tolman has long taken the view that the cognitive activities of rat and scientist had much in common, as indicated by his willingness to apply the then sacred word "hypothesis" to the problem solving efforts of a rat (Tolman & Krechevsky, 1933) and by such asides on the social psychology of the scientist as this: "And once set up, a system probably does as much harm as good. It serves as a sort of sacred grating behind which the novice is com-

manded to kneel in order that he may never see the real world save through its interstices" (1932, p. 394).

It is the purpose of this paper to sketch out a hierarchy of knowledge processes at various biological levels, to examine these for recurrent attributes and differences, and then, considering science as one aspect of a general knowledge process, to make some suggestions of tactics in the attempt to build a social science. Even were it to be complete, this "comparative psychology of knowledge processes" would be but one small part of a comprehensive science of induction. The history of science and the sociology of knowledge represent, for example, well developed parts. Before proceeding, a few comments on this more general area and its relationship to philosophy seem in order. It seems to me that philosophers as scholars, if not in their technical role of logical analysts, have an important and essential part to play in such a total enterprise. If possible, I should like to make this point without blurring the clear distinction between the philosopher's task and an empirical science of induction: indeed, I am dependent upon the distinction for a freedom to participate in the area without a feeling of intruding upon the private domain of a jealous specialty. But it is obvious that the problem area will remain perhaps indefinitely only a quasi-empirical, quasi-scientific enterprise, closer to the descriptive humanities than to the laboratory sciences. Having accepted this condition, it becomes apparent that the major accumulations of speculative discourse and articulation of problems lie in the writings of philosophers. Further, even those philosophers who would draw the line most strictly show a creative interest on both sides of the fence, either as Feigl in specifying the empirical problems remaining around the mind-body problem (1958, pp. 478—482), or as Bergmann in such stimulating, if controversial, psychological asides as ". . . the way our minds are constituted, we could no more do without defined concepts than we could do without deductive inferences" (1957, p. 51). Again without intending to blur the important distinction, it can be observed that philosophers are in very considerable degree specialists in "man the knower," and with this specialization come many *empirical* interests and responsibilities. To renounce these for a purely analytic activity of developing logical calculi disciplined only by internal consistency would be to make philosophy a branch of mathematics. Thus when one selects among potential logical

calculi the one most "intuitively valid" or "relevant", or the one "best clarifying ordinary linguistic usage," one is engaged in a descriptive empirical task (as Meløe, 1958, indicates). This is also the case when one attempts to explicate the inductive rules underlying scientific activity. The search for directly known "primitives" or "givens" partakes of an effort to provide interpretation for a deductive network purporting to describe man as a knower, and would be unnecessary for a purely analytic activity. Taking the professional discipline as it is found today, philosophy is in considerable degree an activity descriptive of man, and is quasi-scientific in this sense. Professionally, the philosopher is certainly agile enough to engage in both analytic and synthetic activities without confusing the roles nor blurring the important distinction between them.

Having thus recognized the need for the activity of professionally trained philosophers in a comprehensive science of induction, it is only fair to note that the subspeciality of a comparative psychology of knowledge processes will be developed here in a manner as little philosophic and as much biologic as possible. However, in an enterprise such as this, it may be thought particularly important to make explicit the philosophic assumptions of which one is aware, and these are sketched in the following points:

> 1) My general orientation I shall call a *hypothetical realism*. An "external" world is hypothesized in general, and specific entities and processes are hypothesized in particular, and the observable implications of these hypotheses (or hypostatizations or reifications) are sought out for verification. No part of the hypotheses has any "justification" or validity prior to, or other than through, the testing of these implications. Both in specific and in general they are always to some degree tentative. The original source of the hypotheses has nothing to do with their validity: in some sense they were originally blind guesses or a chance mutations. Perhaps the position is not essentially different from a critical realism. No more solid or material physical substance need be assumed than that described by modern physics, but this for example, is substantial enough to provide some objects which both reflect light and are impervious to the locomotor efforts of fish and man.
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> 2) Whereas a typical philosophical approach initiates the investigation of knowledge processes by holding all achieved knowledge in abeyance until the very possibility of any knowledge can first be established, the present approach would assume in general all scientific and commonly accepted knowledge, would assume the achievements of modern physics and biology in particular, and

would make use of this cumulative achievement in understanding the knowledge process itself. This knowledge is not assumed to be perfect or incorrigible. Rather, the tactic is one of assuming the correctness of the great bulk of knowledge while skeptically examining one fragment. This is the tactic which has enabled physics to continuously build while at the same time correcting its most basic assumptions.

3) While not escaping an interest in primitive fundamentals to knowledge, no primitives are accepted which are not also appropriate to the knowledge processes of the white rat and paramecium. Man's knowledge processes are undoubtedly more complex and efficient than those of his lowly cousins, but they are not expected to be more primitive nor more fundamental. In particular, no immediate, incorrigible, or directly given knowledge is invoked: At all levels knowledge is indirectly, inferentially, and fallibly achieved.

4) Insofar as the enterprise partakes of an epistemology, it is an "epistemology of the other one," to paraphrase Max Meyer's (1921) famous dictum for a scientific psychology. Naess (Ness, 1936; Naess 1948) has elaborated such a view. In concession to the solipsistic argument, no effort is made to justify "my own" knowledge processes. Rather, the enterprise is one of describing how the organisms under study come to know.

5) At any level, the orientation is one of describing an organism in an environment. In consistency with behavioristic analyses (e.g., Bergmann & Spence, 1944) there is an insistence upon the necessity for separate data series to represent the environment on the one hand (e.g., stimulus-type constructs) and the organism on the other (e.g., response-type constructs) in order that the correlation observed between the two shall not be a tautology. In particular, the rat's learned behavior pattern and the maze are to be distinguished. When this orientation is extended to the scientist in his "maze," the point of view comes dangerously close to what Naess (Ness, 1936) has criticized as "Labyrintherkenntnistheorie". Perhaps the errors of that position are avoided by treating both the organism's-knowledge and the environment-to-be-known as constructs of the scientist of science. The requirement that these constructs be independently definable creates no problem when a rat's knowledge of a maze is under study. But as Naess points out, it does become an important limiting factor when describing the problem-solving behavior of a physicist in a "maze" the description for which one must get from the same physicist. This limit is perhaps avoidable by the study of the problem-solving behavior of scientists at the earlier stages of a now well-developed science. The insistence on the operational independence of "knowledge" and "that-which-is-to-be-known," plus an emphasis upon the indirectness and imperfection of knowing, may characterize my position as a variety of epistemological dualism (Lovejoy, 1930; Köhler, 1938).

6) Although implied in several of the points above, it may

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help the reader to alert him to the use of the hypothesis that the segments of the total environment known by various organisms by various knowledge processes can overlap, and perhaps usually do. A complete physics would not only map many aspects of the environment neglected in the trial and error problem solving of the paramecium or white rat, but will also map the same aspects of the environment as do these animals, e.g., the impermeability and opaqueness of objects, etc.. von Bertalanffy (1955) has eloquently expounded such a point under the term "perspectivism," in qualifying the relativisms of Whorf (1956) and von Uexküll (1920).

SOME OUTLINES OF A COMPARATIVE PSYCHOLOGY
OF KNOWLEDGE PROCESSES

As a psychologist, I start out with the middle steps of a crude hierarchy of knowledge processes, of mechanisms which provide the basis for adaptive locomotion in environments. These include 3. *Blind trial and error problem solving,* characteristic of Jenning's (1906) Stentor and Ashby's (1952) Homeostat; 4. *Learning,* the retention of adaptive response patterns for subsequent utilization, thus abbreviating the trial and error process; 5. *Perception,* visual exploration of potential locomotor alternatives, substituting for overt exploration (Campbell, 1956b); 6. *Observational learning* characteristic of social animals, in which the observing animal learns from observing the outcomes to an exploring animal, and thus profits even from experience that may prove fatal to the exploring animal (Campbell, 1960); 7. *Imitation,* in which a model for behavior is acquired perceptually from the behavior of another organism; 8. *Linguistic instruction* about the nature of the world or the correct responses to be made to it; 9. *Thought,* in which potential locomotions are symbolically checked against a mnemonically represented model of the environment; and 10. *Social decision making,* in which the partial observations of many persons are pooled into a single more adequate model of the environment (e.g., Asch, 1952).

As indicated by the gaps in the numbering, this inventory is incomplete when these psychological processes have been listed. First of all, between some virus-type ancestor and the white rat or precultural man, evolutionary processes provided a tremendous gain in practical knowledge of the world, in the temporal and spatial extent of usable forecasts, in what Spencer (1896) designated the "range of correspondences." The most fundamental knowledge processes making possible this truly stupendous gain are 1. *Genetic mutation and selective survival;* and 2. *Bisexuality*

and heterozygosity which combined with selective survival make possible more rapid adjustments to environmental changes. At the other end of the list can be added 11. *Modern science* (entered as a single process, although containing several discriminable levels). A final category, 12. *Machines,* can also be added to call attention to the knowledge processes embodied in machines, although these form a hierarchy paralleling the biological, from Ashby's (1952) Homeostat at level 3, Walter's (1953) mechanical tortoise to some degree showing the vicarious visual exploration substitutable for locomotor blundering of level 5, and the Newell, Shaw and Simon (1958) logic machine of almost level 9.

Turning to the first level, the process of blind mutation and selective survival must be recognized as a powerful, if slow, inductive procedure. Operating at low efficiency across countless eons, it has produced specific tested and retested assumptions about the nature of reality. These assumptions have been well "probed," but never deductively "proven," to introduce a distinction used in what follows. In addition biological evolution has provided other inductive procedures that speed up, abbreviate, and expand the overall process. These discovered shortcuts include bisexuality, learning, vision, etc., the very workabilities of which in themselves constitute descriptive knowledge of the external world, being mechanisms that would not work in many conceivable worlds. The development of both specific instincts and the general capacity for learning constitute hypotheses as to some degree of stabile inhomogeneity in the energy distributions of the environment, some degree of reidentifiability of settings and opportunities. Vision as a substitute for blind trial and error locomotion (Campbell, 1956b), is made possible by two ecological conditions which no protozoan's phenomenal givens nor theory of knowledge could have anticipated: first that the environment is partially organized into relatively stable discontinuities of mass-energy distributions, i.e., that it consists in part of objects impenetrable by the organism, and in part of penetrable substance; and, second, that objects which are impervious to locomotion in general also reflect or diffuse certain electromagnetic waves. That these reflected waves include the band to which the eye is responsive is in the net no accident, even thought it is a selective accumulation from a large number of little accidents. Transparent water and air were permeable, while transparent plastics such as glass were infrequent in the ecology of evolution. Fog, paradoxical in a way complementary to glass in being penetrable to locomotion but opaque to vision, was not present under water for the basic development of the vertebrate eye in the fish; may in general be eco-

logically rare; is, I am told, more transparent to some waterfowl than to man though an extension of optical sensitivity at one end of the visual spectrum; and is an incongruity being corrected in man by the evolution in radar of a mechanical exploitation of the correlation between imperviousness and reflection."

Lacking an articulation of the inductive process constituted by biological evolution, the older philosophers with the empiricist insight into the preponderant role of induction in man's achievement of knowledge have emphasized the learning process, and have been wary of any assumptions of innate processes, regarding these as covers for teleological pseudo-explanation, metaphysical vitalism, theism, etc. From the present perspective, this precaution seems unnecessary. The evolutionary ecology leading to instinct development and the ecology providing the selection among responses in trial-and-error learning are in large part the same. The processes do not differ in any epistemological fundamental: both are blindly pragmatic and inductive. Furthermore, what is a learned synthetic induction in one species is an inherited innate synthetic induction in another. The disagreements between Helmholtz and Hering on the innateness of specific visual functions, which were once paralleled by the drawing of philosophical lines, seem now epistemologically irrelevant. As a result, the person of positivist, physicalist, empiricist orientation, looking at the tremendously important evolutionary background of man, can today concede more innate endowment in man's knowledge processes than he has previously been inclined to. Recently several persons from quite different starting points have suggested such an evolutionary reinterpretation of Kant's synthetic *a priori* (In greater or less explicitness, these include Lorenz, 1943; Bertalanffy, 1955; Whitrow, 1955; Platt, 1956; and Pepper, 1958, pp. 106—108.) A predisposition toward interpreting events in terms of a three-dimensional space would thus be ontogenetically *a priori,* but not phylogenetically *a priori.* Further, in the sense in which it is now seen, its status as *a priori* gives it no truth status different from that of any other much used inductive inference — but by the same token its selective survival from among countless other mutation-combinations provides an inductively strong credence base. Similarly with regard to causality: to some extent, Hume and Kant can be seen as disagreeing over the psychological fact of whether or not the tendency to perceive cause-effect relationships is learned within the person's own lifetime, or is rather a phylogenetic "learning". Michotte's (1946) research ties up the process with event perception in general, and finds the experience automatic and indivisible. And from the

biological point of view, event perception, as in the perception of motion, is at least as primitive as the perception of discrete objects. Having myself (1960) identified the perception of causality as the phenomenal counterpart of what the behaviorist records as stimulus association through contiguity in conditioning, I would tend to regard it as just as innate as the capacity to learn. But in any event, the argument as to innateness or learnedness is now a problem of psychological fact and of no epistemological relevance.

Except for his relative neglect of the blind variation and selective survival mechanism, Spencer long ago anticipated this development, which must stand as one of the sound contributions of this overproductive man, overaccepted in his own day and overrejected in ours. His point of view can perhaps be most economically presented in the words of a contemporary, Höffding (1900). (The italics are for the most part my own:)

> With regard to the question of the origin of knowledge Spencer makes front on the one hand against Leibniz and Kant, on the other against Locke and Mill. He quarrels with empiricism for two reasons: — firstly, because it does not see that the matter of experience is always taken up and elaborated in a definite manner, which is determined by the original nature of the individual; secondly, because it is lacking in a criterion of truth. We must assume an original organization if we are to understand the influence exercised by stimuli on different individuals, and the criterion by means of which alone a proposition can be established is the fact that its opposite would contain a contradiction. In the inborn nature of the individual then, and in the logical principle on which we depend every time we make an inference, we have an *a priori* element; something which cannot be deduced from experience. To this extent Spencer upholds Leibniz and Kant against Locke and Mill; but he does so only as long as he is restricting his considerations to the experience of the individual. *What is a priori for the individual is not so for the race.* For those conditions and forms of knowledge and of feeling which are original in the individual, and hence cannot be derived from his experience, have been transmitted by earlier generations. The forms of thought correspond to the collective and inherited modifications of structure which are latent in every new-born individual, and are gradually developed through his experiences. Their first origin, then, is empirical: the fixed and universal relation of things to one another must, in the course of development, form fixed and universal conjunctions in the organism; by perpetual repetition of absolutely external uniformities there arise in the race necessary forms of knowledge, indissoluble thought associations which express the net results of the experience of perhaps several millions of genera-

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tions down to the present. The individual cannot sunder a conjunction thus deeply rooted in the organisation of the race; hence, he is born into the world with those psychical connections which form the substrata of "necessary truths" (see *Principles of Psychology*, pp. 208, 216; cf. *First Principles*, p. 53. "Absolute uniformities of experience generate absolute uniformities of thought"). Although Spencer is of opinion that the inductive school went too far when they attempted to arrive at everything by way of induction (for, if we adopt this method, induction itself is left hanging in the air), yet, if he had to choose between Locke and Kant, he would avow himself a disciple of the former; for, *in the long run, Spencer too thinks that all knowledge and all forms of thought spring from experience*. His admission that there is something in our mind which is not the product of our own *a posteriori* experience led Max Müller to call him a "thoroughgoing Kantian," to which Spencer replied: "The Evolution-view is completely experiential. It differs from the original view of the experimentalists by containing a great extension of that view. — *But the view of Kant is avowedly and utterly unexperiential*" (Höffding, 1900, pp. 475—476). . . . . "It is of no small interest to notice that John Stuart Mill, who at first demurred at Spencer's evolutionary psychology, afterwards declared himself convinced that mental development takes place not only in the individual but also in the race, by means of inherited dispositions. He expressed this modification of his view a year before his death in a letter to Carpenter, the physiologist (quoted in the latter's *Mental Physiology*, p. 486)" (Höffding, 1900, pp. 457—458).

Among the twelve and more knowledge processes there are both common features and significant differences. Certain processes are inventions representing especially striking gains in efficiency. Vision, while opportunistically exploiting a limited ecological coincidence, does so with such marvelous efficiency that it becomes in many ways prototypic of all knowledge processes. Thus, although from the biological perspective vision is a substitute for the more basic locomotor exploration and is much more inferential, indirect, and "improbable" than is locomotor exploration, it is so much more efficient that it has been taken by many on introspective grounds to be the basic and most direct process. (While emphasizing the indirectness of vision, one can none the less recognize it as fundamental to all of the higher modes, and also to learning, when recognition of objects is involved.) Like vision, language too is such an efficacious device that it has been given a total preponderance in some descriptions of the knowledge process. Certainly the twelve categories should not be regarded as of equal value, nor as a series of steps

of equal gain, nor as discrete stages in which the lower are abandoned once the higher have been achieved, nor, of course, is the listing complete.

BLIND VARIATION AND SELECTIVE SURVIVAL AS A RECURRENT THEME

Not only do the different processes differ in these important ways, but there are also recurrent similarities, analogous processes occurring at various levels. Most pervasive of these is blind variation and selective survival. Ashby (1952), Pringle (1951), Baldwin (1900, pp. 174—191), Asch (1952, pp. 96—98), and others have noted the essential similarity of inductive process between natural selection in evolution and trial-and-error learning. While the parallel is obvious when pointed out, it is perhaps the key insight in this whole reorganization of perspective on knowledge processes. Ashby's presentation of the inevitable evolutionary tendency for the stable to replace the unstable is particularly impressive, and provides a linkage to crystal formation and counter-entropic growth processes in general, e.g., as described by Schrödinger (1945). The common features of an inductive knowledge process as illustrated in evolution and learning are these: 1. a mechanism for variation (of structures, or responses, etc.) 2. a selection process whereby certain variations are preserved and others lost according to stable criteria (differential survival in evolution, pleasure-pain reinforcement in learning) and 3. a mechanism for preserving or propagating the surviving variations (the genetic mechanism in evolution, memory in learning). In its general form, this seems to be the available deterministic model for all processes that are teleological and adaptive at a more molar level (Campbell, 1956b). There are, of course, important differences in each application. In a gross way, the criteria of selection involved in biological evolution are external to the organism or to the species (note partial exceptions in the selection of secondary sex characteristics and parent-infant feeding instincts). In learning (and in developmental morphology, Spiegelman, 1948) the criteria are internalized, the sense receptors for pleasure and pain acting as representatives of general truths about organism-environment relations achieved initially by the mutation-selection process. Such internalized criteria are much more precise, have a much higher "selection ratio" than does mutation-selection, but also represent less direct encounters with environmental realities and thus are a less fundamental knowledge process. Blind variation and selective retention is none the less involved in learning, being a feature of all

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comprehensive learning theories, including those of Gestalt descent (Campbell, 1956a).

A trial-and-error theme recurs again and again in the various knowledge processes. Perhaps it is oldest and most recurrent in descriptions of the thought process. Alexander Bain (as cited by Woodworth, 1938) used the phrase "trial and error" to describe human thinking long before Morgan, Hobhouse, and Thorndike applied the term to animal learning or problem solving. Recent treatments of thinking, as reviewed by Sluckin (1954) for example, reiterate the same point. Even in patently deductive processes such as logical and mathematical proof, the introduction of novel solutions is in part at least a trial-and-error process (Quine, 1947, pp. 5—6; Polya, 1945, 1954). Baldwin (1906, p. 169) has described imitation or motor mimicry as a process in which the sensory input provides a criterion (e.g., a heard tune) which one attempts to match by a motor trial-and-error process. The vicarious trial-and-error aspects of observational learning have been made explicit in the initial presentation of the process above, and by Herbert and Harsh (1944). Descriptive accounts of the process of science abound in reference to such factors (e.g., Ritchie, 1923, pp. 4—5; Jennings, 1930, pp. 186—187; Cannon, 1945, pp. 68—78; Aubert, 1959). Pepper, (1958, p. 106) has a subchapter headed "Scientific Method as Systematized Trial and Error." Merton's frequent use of the concept of serendipity (e.g. Merton, 1949) has made the role of chance discovery explicit for social science (but my citing him here does not do justice to his emphasis on the encountering by chance of solutions to problems one did not intend to be working on). While the case is far from obvious at the level of visual perception, I have attempted to make the point that not only is vision a substitute for locomotor trial and error, but that it also embodies a selective survival component (Campbell, 1956b). Pumphrey (1950) has presented a similar case for space perception mediated by the lateral line organ of the fish. In both cases, it helps to use the analogy to radar, in which an exploration of the locomotor possibilities of the environment is achieved through the selective reflection of blindly emitted radio pulses, each pulse being regarded as a substitute for an exploratory locomotion of the whole ship in the given direction.

It is tempting to infer that something of a blind variation and selective retention process is the basic ingredient in all inductive extensions of knowledge. Insofar as one's guesses are correct other than by happenstance, one is making use of already accumulated knowledge, however approximate. Theory in science reduces the tremendously costly blind

exploration to the minimum, but in its valid aspects cannot do more than spell out the implications of what is already known. Erroneous theory may, of course, have the heuristic value of leading to the exploration of quite novel possibilities, and thus of encouraging a wider range of "variations" than would otherwise have been tried.

SOME GENERAL IMPLICATIONS OF THE SELECTIVE SURVIVAL THEME
FOR THE CONSTRUCTING OF A SOCIAL SCIENCE

In the preceding pages, outlines and fragments of a comparative psychology of knowledge processes have been offered. In the remaining sections, an effort will be made to apply this perspective to the problem of achieving a social science. This section and the two which follow build upon the trial-and-error theme. The last two make use of analogies from the visual process.

If science be viewed as a process of exploration and adaption analogous to mutation-selection in evolution, or to trial-and-error learning, these three basic requirements must be met: 1. variation in trial, hypothesis, or prediction; 2. a machinery for testing hypotheses; and 3. machinery for preserving and duplicating the hypotheses which survive the testing. From these requirements, certain truisms can be drawn: In general, science will grow only where growth is possible — in this sense, "science is as opportunistic as a bacteria culture" (Campbell, 1957, p. 310). In specific, no science can grow without machinery for selecting among theories or guesses, and the criteria for selection must be consistent. Science will develop fastest where the selective apparatus is sharpest, and thus often grows unevenly, around gadgets such as the microscope, the column of mercury in the glass tube, the cat's fur and amber, and, in the social sciences around fortuitous circumstances that make experimentation possible. So important is the selection machinery that for building a social science, a trivial problem which is amenable to the experimental probing of hypotheses is to be preferred to an important problem area where selection from the glut of alternative theories is impossible. With the tremendous pressures for an immediately useful social science, this has been, and will continue to be, a difficult decision to accept.

Given the laboratory for rejecting hypotheses, science will develop most rapidly when the widest range of guesses is being tried. If the testing process is very expensive, then vicarious testing through theoretical integrative efforts utilizing the empirical base of folk-wisdom may

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be used as a preliminary, but it cannot carry the whole load and eventually experimental confrontation is required. Where experimentation is cheap, as in laboratory psychology, some considerable blind variation of parameters without benefit of theory seems eminently justified. While observers of the process of science have frequently called attention to the role of the fortuitous in scientific breakthroughs (e.g., Barber, 1952, pp. 191—206), graduate students in the social sciences are in general not exposed to this sanction, nor allowed openly to implement it. Instead, the students learn their strategy of science-making from philosophers of science who, if asked, would disclaim that their analyses were intended for this purpose. They have therefore been led to a strategy in which explicit definition and deductive-postulational theory take temporal precedence over experiment or the discovery of laws. They have been led to feel guilty and apologetic for exploratory experimentation and discoveries not sanctioned by prior theory. Perhaps a theory of knowledge processes in which blind exploration plays a fundamental role (even if recognized as more cumbersome and less efficient than shortcuts through theory when these work) will redress the balance on this point.

This blind or pre-theoretical *experimentation* should not be confused with the blind theory-free "fact" collecting common in the social sciences, which results in a collection of uninterpretable instances, and isolates no functional laws because all parameters vary simultaneously, all observed differences thus being "overdetermined" and subject to subinfinity of possible explanations among which there can be no disciplined choosing.

In general, the rats which emit the widest range of responses learn fastest, and the species with the most rapid mutation rate evolves most rapidly, as long *as the mutation rate is not so great as to jeopardize the already accumulated knowledge.* Note that Point 1, *variation* and Point 3, *preservation* and *transmission,* are at odds, and that a minimax solution between them must be obtained. For a well established science, or a very highly evolved organism, the greater weight must be given to transmission, to indoctrination, to training, to passing on what is already known lest that be lost in devoting too great effort to search for innovations. An undeveloped organism or science has less to lose by over-exploration (although its less complex achieved base greatly restricts the variety of innovations possible). Were these the only considerations, it is clear that the proportion of graduate student effort devoted to novel experimentation, as opposed to indoctrination, should be greater in the social sciences than in the physical. That the reverse is so should give

us reason to question current practice. One excuse for the heavy proportion of indoctrination time in the social sciences is that our store of accumulated wisdom, while not larger in terms of total facts *predicted,* is much harder to transmit, takes longer to memorize and more books to record. Further inspection of the details to which this argument refers will show, however, that this is so in large part just because the social sciences do not yet embody an achieved scientific status and lack the enormously efficient crystalizations of effective predictions which adequate theories represent.

The recognition of the important role of trial-and-error processes in expanding science, and the realistic (albeit "hypothetically realistic") orientation which accompanies such recognition, puts in perspective several narcissistic features of social science making. One of these features is our attitude of disappointment when we learn that someone else has made the same discovery as we. Such disappointment is appropriate for professional "creators" such as poets, novelists, and modern painters, as it is a sign of professional inadequacy. But for persons whose profession is "discovery", not "creation", such independent confirmation shows professional *adequacy,* through demonstrating the validity of one's achievement (insofar as the replication was independent and insofar as both replications were advances, rather than restatements of beliefs already existing in the shared culture). If many persons are engaged in attempting to add to the valid accumulations of a science, then blind trial-and-error, or creative insight, will lead many to the few available solutions at about the same time, and the solution is no less an advance or and achievement for the multiplicity of its discovery. (In mathematics, the discipline of internal consistency plus the accumulation of common achievement, likewise makes independent discovery something to be expected and a sign of validity.)

Another symptom of an inappropriate narcissism which I see in the graduate students in the social sciences, as contrasted with their peers in the biological or the physical sciences, is a greater demand for a narcissistic originality in problem selection and method. But perhaps this is desirable if, in fact, we have as yet no striking achievements worth building on. And as Merton's (1957) study of priority disputes in the history of science shows, narcissism has not been lacking in the successful sciences during their most creative phases, and indeed may play an essential role in maintaining the social institution of science.

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In the social sciences today we are faced by a group of interrelated problems. On the one hand, scientific literature is being produced so rapidly that an active scientist cannot keep abreast of his own field. On the other hand, there is a shortage of journal space for worthy contributions; publication backlogs soon double if editors are not vigilant; and in many fields, there exists a "prepublication" literature of duplicated research reports, convention addresses, and dissertations as large as the published literature. On the other hand, we note younger scholars publishing in ignorance of relevant prior research; we note inadequate bibliographical search in supposedly complete literature reviews; and for ourselves we recognize the practical impossibility of basing our own research upon all of the directly appropriate prior studies. The literature has become, or will soon become, so vast that even though a given study already be published, a literature search to discover it may be more arduous and costly than to "needlessly" repeat the experiment. In the face of this pressure, our journal editors often are reluctant to publish negative results on novel problems. Application of the selective survival model for the accumulation of knowledge seems to provide some justification for this decision. If the enterprise of science be conceived of as a process of guessing at possible laws and seeing which survive this testing, the records of this process could include both the guesses that survived and those that did not. Instead, the suggestion is that only the records of what "works" be kept, with no recording of what was tried and produced no laws. While there are many disadvantages to the decision, our situation is such that it should seriously be considered.

If we look to other knowledge processes, now seen as to some degree analogous through the perspective of the comparative psychology of knowledge processes, we find that such a superficially wasteful tactic occurs on several levels. It is perhaps most strikingly characteristic of mutation-selection. The present generation contains in its genes a record of the variations that were adaptive (or were not maladaptive). The record of the totally unsuccessful variations is lost with the organisms which embodied them. As a concomitant of this onesided record keeping, the species keeps on making the same old mistaken mutations again and again (although, through the greater reproduction of the more successful variants, these become in each generation a smaller proportion of the total genetic pool). In spite of this apparently wasteful repetition of old mistakes, the accumulation of adequate knowledge steadily ad-

vances. Insofar as Ashby's (1952) Homeostat stores any wisdom, it records only its successful steppingswitch combinations, and these are preserved by being left intact. Ashby's most general principle, that the stable tends to replace the unstable, implies that the trace of each of the unstable combinations is lost, while a stable combination is self-perpetuating. Bruner *et al.* (1956) emphasize the overwhelming memory load introduced by problem-solving strategies which require memory for hypotheses ruled out as well as hypotheses that have withstood a given probing, and they have called attention to the availability of adequate strategies which do not require memory for the disproven. And in laboratory studies of concept formation, persons have long been known to profit mainly from positive instances.

Hovland (1952), in an analysis of the roles of negative and positive instances in problem solving, has called attention to an ecological feature in the tasks typically employed which makes negative instances intrinsically less informative than positive instances. Of all of the possible concepts that the discriminable features of the stimuli make possible, only a very small proportion are called correct by the experimenter, leading to an imbalance in which a single positive instance reduces equivocality much more than a single negative instance. It may be argued that the strategy of neglecting wrong trials should be limited to such settings. In retrospect, it can be noted that the ecology of organisms during the course of evolution was such. There are many more ways of constructing an organism that won't work than one that will. In retrospect, this also seems true of the ecology of the physical sciences. The social sciences are no doubt operating in a similar setting.

It seems reasonable that an achieved science of science would provide rules for an editor of a research journal to follow. A recommendation coming from a preliminary fragment of that science is justifiably suspect, but can be offered for consideration nonetheless. The suggested rule is this: reports on investigations of new variables and novel combinations of variables which find no clearcut relationships should *not* be published, unless such relations are clearly predicted by well-established theory. From the standpoint of the individual scholar, such a rule seems highly unjust. Particularly in consideration of the role of publications in the economics of scholarly life, does such a rule seem unjustly to penalize the scrupulous and thorough scholar, and to play into the hands of the opportunistic and careless person using small samples and inadequate controls. Noting how small a segment of our research gets even submitted for publication, a cynic might estimate that with authors and

editors using the .05 level of significance we in psychology are publishing only one-twentieth of our experiments. The suggested policy would seem to operate to increase such biases. However, the correction for the bias in our literature toward novel false positives does not lie in publishing novel negatives, but in increasing the requirements for what we regard as evidence establishing a novel law. The level of significance which we employ is after all an estimate of the ecological setting of our science, to be revised as we learn more about that environment. Since we in social psychology at least are now more troubled by false positives than by false negatives, and since we are testing hundreds of thousands of hypotheses each year, a shift of the acceptable level of significance from .05 to 001 would seem to be reasonable.

The evolutionary analogy may lead us also to see a justification for certain trends in science as a social system which we might otherwise judge unworthy. In science, as in evolution, growth must opportunistically be based upon past successes, and when a novel approach offers exciting results, it is both natural and proper that a fad or a bandwagon effect takes place, with many scientists shifting to the new problem area and method. This very tendency makes it certain that a false positive will not long go undetected. Similarly, the perspective justifies those who, being the possessors of an exciting and productive line of investigation of their own, continue to build upon and exploit their own past successes in neglect of the research of others. The opportunistic nature of life and of science may not always be admired, but it should not be denied.

DIAGNOSING DISPOSITIONS

In the course of an effort to integrate the concept of *social attitude* with other concepts in psychology dealing with acquired behavioral dispositions (Campbell, 1960), it become desirable to provide a paradigm for the operational delineation of terms like *attitude* or *habit*. In psychology, too many so-called operational definitions merely specify usual antecedent conditions, and do not point to the distinguishing characteristics of the resultant disposition itself. In particular, a paradigm was wanted for attitudes or habits diagnosed by an observer who had *not* watched their acquisition and had *not* seen the environment in which they were learned. The following parable proved to be useful for this purpose, and employs the perspective of a comparative psychology of knowledge processes.

Let us pose to an animal psychologist at one university the problem of diagnosing the habits of an aged and experienced rat shipped to him from another laboratory, with no information as to past environments and reinforcements. The process would be a hit-and-miss, trial-and-error process. Knowledge that the rat shared some common culture, i.e., that it was a university psychology rat, would make the selections of apparatus somewhat less random. The animal would be placed in a Skinner box while buzzers buzzed and lights flashed, and any combinations that produced increased lever pressing would be taken as symptoms of some habit. The rat might be placed on a Lashley jumping stand while various colors and designs were used as discrimination cards, and if any jumping occurred an effort would be made to find to which card the jumping was most consistent. In similar fashion, T mazes and runways would be explored. With luck, habits, or significant contingencies between stimuli and response would be encountered. But no matter how clever the research, there would still be the possibility that important, highly specific, and stable habits of the rat would go unnoticed by the diagnostician.

The initial definition of stimulus and classification of response are the experimenter's. They represent classes of objects and classes of behaviors which the experimenter can consistently discriminate (and which he guesses the animal can also). Once he finds some evidence of stimulus-response consistency on the part of the rat, he would typically start varying stimuli and varying his classification of movements in order to approximate more closely the optimal description of the habit. Thus if he found that the rat jumped to a yellow circle he would start varying both the color and the shape to find what maximized the contingency. Likewise, he would try and discover the optimizing classification of response, whether as muscle contraction or locomotor achievement, etc. Although by this trial-and-error he might come closer to the "rats own" definition of the situation, the final classifications would still be in the scientist's terms and would be limited to classifications which the scientist could make. (He could of course add mechanical aids to his own senses, as in the diagnosis of the responsiveness of various insects and mollusks to the plane of polarization of light.)

Even though the definition of stimulus and response are in the experimenter's language, there is still a verifiability to his diagnosis, through the simple actuarial matter of a significant contingency of stimulus and response. He can, of course, never claim to have reached the optimal classification. But if two diagnosticians disagree, the actuarial approach can say which is the more efficient, which is presumably closer to the original acquisition conditions, or to the "rat's own" definition of stimulus and response.

Note a general limitation on the process: the diagnosis of the rat's habit is only possible insofar as the rat and the scientist

overlap to some extent in their classifications of the environment into entities. Were the rat indeed to be responding to constellations of atoms sharing no boundaries with the constellations that the scientist was able to discriminate, the diagnosis of the rat's habits would be impossible. Similarly, learning of the language of another, whether as a child or as an adult, would be impossible if there were not such overlap in the discriminable contours of matter classified in linguistic usage. Thus a color-blind person never completely learns the language of those who discriminate between red and green. The fact that learning a strange language is possible at all sets an empirical limit to the extent of a Whorfian relativism.

While there is not space here for expanding on the implications of this paradigm for attitude and personality diagnosis, it does present another illustration of the ubiquitous necessity of a trial-and-error aspect to knowledge processes. The parable can also serve as a paradigm against which to refer the alternatives of nominalist *versus* realist interpretations of scientific achievements. It is clear that at every stage, the definitions of stimulus and of response are the inventions or conventions of the scientist. But the contingencies discovered are not therefore arbitrary fictions, and through the iterative process of optimizing the definitions, the final definitions are less "arbitrary" than the original ones might have been, in that they have been selected "by the rat", as it were, from a large array of possible definitions offered by the diagnostician. Bergmann (1957) presents a comparable point of view which can be approximated in these terms: definitions (and operations) are invented, laws relating the operations are discovered. Fruitful definitions are those that produce economically stated laws. The final definitions employed by a science are thus no longer the undisciplined creations of a scientist, but are rather a highly select set, selected from among a very large array of alternates.

INFERRED EXTERNAL ENTITIES AND THEIR VERIFICATION

At some level in the hierarchy of knowledge processes, the organism tries out the strategy of hypothetically inferring stable entities and processes external to itself. This step may be thought of as occurring first with visual space perception, although there are reasons for putting aspects of it earlier, and also of ruling it out for the simplest forms of eyes. For example, Piaget (1957) finds such hypostatization to some degree absent for the very youngest infants. In any event, the shift in

the mode of storage of knowledge and of acquisition of new knowledge is a dramatic one. Below this level, the knowledge embodied in instinct and habit can be regarded as organized in terms of an inventory of specific muscle-contractions to be made to specific receptor-cell activations. Above this level, knowledge becomes distal (Brunswik, 1952, 1956), and acquires a reference to objects with specific locations in an extended space beyond the organism's skin. This must of necessity introduce something on the order of central nervous system modeling of the external environment (Craik, 1943) which makes possible multiple intersubstitutable diagnostic procedures (the "vicarious functioning" of Brunswik, 1952, 1956). That such a highly presumptive and indirect process works is a function of the kind of environment in which the organism locomotes in its search for food and safety. The presumptive reifications involved are, for this ecology, well probed and satisfactory to a high degree of approximation. They make possible efficient loco-motion from such a subinfinity of organism-environment juxtapositions as to certainly have outurn the storage capacity for specific sense-receptor—muscle-contraction rules leading to the same behavior.

It seems likely that the higher cognitive processes developed from vision, and contain residues of its dominance. Visual knowing is strongly given to reification, to the hypostatization of real external entities con-taining attributes beyond those given to vision. Such a realistic bias has been amply justified in the visual diagnosis of middle-sized objects: the inferred attributes when checked by other knowledge processes, such as touch or locomotor effort, are almost always confirmed. However, when a central nervous system machinery based upon this experience is used in other ecological settings, a tendency to reification might be carried over in a way unjustified in this novel ecological setting. Thus symptoms of realism or reification in construct building in the social sciences might be justifiably suspect.

On the other hand, the vividness and phenomenal directness (in spite of physical mediateness) of our visual diagnosis of middle range entities has led many philosophers to grant to the constructs of the visual machinery a special epistemological status not granted to the construc-tions of other knowledge processes, no matter how well established. In doing this, the clues by which the visual construction of external objects is achieved are left unspecified, so that even were the same criteria of external objectivity to be met, a comparable degree of "reality" would not be granted. In this fashion, the extreme non-reifying nom-inalist at the level of scientific constructs is apt to be an uncritical direct

realist at the level of the visual knowledge of common objects. While the electronic microscope and the radio telescope are providing helpful transitions (few would say that an invisible radio star was less real than a radio-silent visible one), the attitude still occurs among the methodologists of social science.

The cure for both of these unfortunate by-products of visual vividness is to make explicit the kinds of evidence involved in the visual construction of external objects, and then to set up formally equivalent operations for other types of hypothesized entities. The cure is not a total abstinence from theorizing about external processes and entities which are only indirectly known — the cure is rather insistence upon procedures for verifying constructs, for probing or checking the implications of such hypostatization.

My most explicit effort to apply this orientation is to be found in a paper entitled "Common Fate, Similarity, and other Indices of the Status of Aggregates of Persons as Social Entities" (Campbell, 1958). There is space here for only the briefest sketch of its argument:

> The clues used by the visual perceptual system in diagnosing entityhood have been described by the Gestalt psychologists as proximity, similarity, common fate, and continuity or pregnanz. The latter can be generalized into a criterion of closed boundaries, the first three can be interpreted as criteria for generating boundaries. Labeling component fragments of the stones in a gravel pit and stirring frequently, one could diagnose the separate thinghood of each stone by the high common-fate coefficients among its constituent parts, and by the lower common-fate coefficients between a part of one stone and a part of another, the decline in common fate coefficients producing a boundary line in a cluster-analysis fashion. Such common-fate coefficients could also be applied to the individuals in an aggregate of persons containing an hypothesized social entity plus hypothesized non-members, and a measure of entityhood achieved of comparable epistemological standing to that of the stone. In parallel fashion, the possible utility of indices based upon similarity and proximity are examined. The many troublesome details of feasible indices must be elided here. The general conclusion is that the status of aggregates of persons as discrete objects or entities can be examined on criteria comparable to those employed in the diagnosis of middle-sized physical objects. In neither case will common-fate indices ever be unity — stones can be broken, etc. In general, some social groups can be stated to be entities upon the same general grounds upon which sticks and stones are so diagnosed. However, the degree of entityhood — the relative height of the intra-entity coefficients

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to the inter-entity values — would probably be lower for most social groups than for most stones. The coincidence of boundaries based upon several different diagnostic criteria would in addition be less frequent for social entities than for physical ones — and it is upon such multiply confirmed or multiply diagnosable boundaries that the economy of reifying a stone or a group as an entity depends.

If a science of social groups is to be able to usefully employ the attitudes of discovery, problem solving, independent confirmation, and validation of constructs which characterize the successful sciences, such multiple confirmability of boundaries must at least occasionally be achieved. From the evolutionary perspective, it seems clear that the development of vision was predicated upon an environment populated with stable, solid, clear-cut entities — that it could never have developed in a world of fuzzy-edged amoeboid clouds or in a completely fluid homogeneous material space. By analogy, one might guess that the development of any science is predicated upon the discovery of such natural nodes of organization, upon stable discontinuities. If the discreteness and multiple-diagnosibility of entities at the social level turns out upon examination to be lacking, then the possibility of a social science representing a separate level of analysis from the biological or psychological may well be eliminated, but this is not to be decided on the grounds of *a priori* analysis.

CONVERGENT AND DISCRIMINANT VALIDATION OF PSYCHOLOGICAL
TESTS THROUGH A MULTITRAIT-MULTIMETHOD MATRIX

Like the previous point, this final illustration of a methodological principle deriving from a comparative psychology of knowledge processes makes use of the model of vision. But whereas the previous topic employed monocular diagnostic cues, the present one analogizes from the resolving power of binocular parallax in the perception of distal objects. Like the previous illustration, the technical details of the methodological requirement are published elsewhere (Campbell & Fiske, 1959). However in this case the original article does not make explicit the relationship of the criterion suggested to a comparative psychology of knowledge processes. This restraint (imposed by limitations of space, the intended audience, and by a coauthor who based his conviction as to the correctness of our position upon other grounds) is partially corrected here.

The question of the validity of psychological tests, and in particular the question of their construct validity (Cronbach & Meehl, 1955), is clearly a problem for the realist, for the reifying, hypostatizing type that had something in mind which he wanted to measure before he constructed the test, and who, now that the test is constructed, has qualms that it may not be measuring that something perfectly, or even qualms that there may be no such thing as what he was trying to get at. For the thoroughly consistent nominalist, for the extreme single-operationalist, the notions of imperfect measurement, of illusory or non-existent constructs, are lacking. From the evolutionary perspective one can understand how such reifying, hypostatizing animals as the realist personality testers came to be. But as already emphasized, the cure is not to suppress these animal instincts and drive them into the unconscious only to have them slip past the censor in highly disguised form, but rather to provide an arena in which the inadequacies and unjustified connotations of the hypostatizations are made apparent, as well as whatever justification they may have. To help confront the personality test designer with the degree of validity of his reifying imputations about his test scores, Fiske and I recommend this seemingly arbitrary and dogmatic requirement: To validate a single psychological test, a matrix is required representing all of the intercorrelations among at least two independent methods of measurement each applied not only to the trait in question but also to at least one additional, dissimilar trait.

As are most suggestions for eliminating the unverified or unverifiable baggage of presumptions surrounding scientific and common sense terms, the procedure is a kind of operationalism. The requirement calls for a *multiple, probing, delineating* operationalism (e.g. Campbell, 1954), or what Garner (1954, 1956) has very aptly called a *convergent* operationism. But it stands in disagreement with a rigid *single* operationalism or an extreme *defining* operationalism when they are recommended as practical operating procedures for the working scientist. Rather, as in any knowledge process which hypothesizes entities and processes beyond the skin of the organism or beyond the meter readings of the science, all indicators are understood to be both indirect and fallible, and to achieve their estimate of the distal construct only in conjunction with the involvement of other separate processes. Thus while certain complexly compensated meters of physics may provide relatively pure measures of single constructs, no single operation is taken as perfectly defining of the distal construct. Instead, the firmness and relative unequivocality of our knowledge of distal constructs comes through a triangulation

from two or more operations, no one of which has priority as *the* criterion or *the* definition, and no one of which would be unequivocal without the other.

Were visual perception and memory to operate according to the extreme operationalist position, they would avoid any hypostatization of external entities. Each specific pattern of retinal cell excitation would be taken as an ultimate defining term. Instead of showing this restraint, the normal mental processes infer from the visual pattern remote external entities mediately known. But when a single eye views a scene from a single vantage point, the retinal pattern is equivocal for the purpose of inferring such distal objects, for there is a subinfinity of possible external events which would generate the same retinal pattern. Consider the implications of the Ames distorted room (e.g., Beardsley & Wertheimer, 1958, pp. 433—443). Viewed by a *single* eye from a specific point of vantage, it seems rectangular — but the important thing to note is that from this particular point of viewing there is a large family of *distal* hexahedrons which would produce the same retinal pattern. The distal interpretation of the proximal stimulation is equivocal. A retinal excitation pattern taken as an ultimate defining operation rather than as a mediating cue has no such equivocality. This equivocality is removed, or greatly reduced, when a *second* point of viewing is introduced, as through the monocular parallax of head movements, through binocular parallax, or, in the Ames room, through exploring the room's shape with one's hand or a stick. The number of singular constructs about externals which will account for the readings from both sets of operations is, relatively, very limited.

In a similar way, any single meter or any single psychological test produces readings which are equivocal when employed to infer the status of constructs not exhaustively defined by the measure, or when used as symptoms of something that they do not perfectly represent. The joint employment of maximally *independent* methods focused upon the same construct greatly reduces this equivocality, greatly reduces the number of tenable rival interpretations for *both* sets of measures, and thus indicates the degree of success of the constructual enterprise. Fiske and I have referred to this aspect of validation as *convergent* validation. It constitutes a *methodological triangulation* (Campbell, 1956c) analogous to what Feigl (1958) has referred to as the "fixing" of abstract concepts in science by a "triangulation in logical space". One might well estimate that *all* of the useful, verified, abstract concepts in the successful sciences are thus established.

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The second aspect of the analogy refers to a requirement which Fiske and I call *discriminant* validation, the requirement that specified correlations in the multitrait-multimethod matrix be zero, or at least be lower than the validity values. This requirement has particular importance in psychological testing because of the demonstrated strength of method factors such as halo effects, response sets and apparatus factors. I see in this requirement, and in our means of implementing it, an analogy to the role which contours play in guiding binocular fusion, and in thus making binocular parallax usable. To clarify this point, let us consider visual triangulation as it would exist if each eye consisted of a single retinal cell, a single rod or cone, for example. Or let us try to duplicate a triangulation process with two mechanical eyes each consisting of a single photo cell, or a fixed radar beam that does no scanning. Or consider a demonstration more feasible in the psychological laboratory in which each eye's view is reduced to a very small area through a reduction screen. Under such conditions, triangulation and binocular parallax would be of no use, false confirmations could not be told from true ones, nor would the retinal evidence provide the guide for the convergence or the aiming of the eyes which it normally does. Analogously, Fiske and I hold that a single validity coefficient, i.e., an isolated correlation between efforts to measure one trait by two different procedures, is uninterpretable. Binocular parallax will not work unless each eye is multiple-celled enough to pick up both thing and non-thing, to provide both figure and ground, to provide contours which can be checked for similarity and thus guide fusion. The analogous background for the interpretation of the validity coefficients is provided by the inclusion of *several traits* in the validation matrix, the most original part of our requirement. Through these, particularly as they generate the heterotrait-heteromethod correlations, the context is provided in which validity coefficients can be interpreted as confirming or disconfirming. (In some of our examples, validity values of .30 indicate some successful triangulation, in others .30 values indicate total failure, depending upon the size of the surrounding heterotrait-hetermethod values.)

While triangulation greatly reduces equivocality, it is not, of course infallible. The Wheatstone stereoscope in itself demonstrates the condition under which the perceptual system can be led to erroneously infer a single entity when two drawings are actually present. The extreme rarity of such complex coincidences as the stereoscope in the normal ecology of the use of the eye makes this exception trivial in practice. It does however alert one to the absence of any deductive certainty or

finality of proof for the distal constructs "confirmed" by triangulation. They only become much more likely through a successful confirmation.

It is perhaps surprising how infrequently discussions of scientific method mention the great importance of confirmation by highly independent procedures, or the fact that remote confirmations are more convincing than ones which involve two very similar apparatuses. The principle does receive occasional mention, however. Margenau comes close with his emphasis upon "bootstrapping" and upon science as an extension of perception, and, in context, by statements such as this: "The greater distance between C_1 and C_2, the greater the departure from triviality in the circuit" (1950, p. 103). Pepper's (1942) concept of structural (as opposed to multiplicative) corroboration seems relevant. Northrop has the concept of "epistemic correlation" which "joins a thing known in one way to what is in some sense the same thing known in another way" (1949, p. 119). Ayer shows commonsense awareness of the requirement, although no formalization of it, in this aside: "if these sources are numerous and independent, and if they agree with one another, he will be reasonably confident that their account of the matter is correct" (1956, p. 39). Feigl (1958) in his long chapter on the mind-body problem refers in several places to "triangulation in logical space," and it is to be hoped that we can look forward to his providing us with a full development of this most important concept.

References

ASCH, S. E. *Social psychology*. New York: Prentice-Hall, 1952.

ASHBY, W. R. *Design for a brain*. New York: Wiley, 1952.

AUBERT, V. Chance in social affairs. *Inquiry*, 1959, 2, 1–24.

AYER, A. J. *The problem of knowledge*. New York: St. Martin's press, 1956.

BAKAN, D. Learning and the scientific enterprise. *Psychol. Rev.*, 1953, 60, 45–49.

BALDWIN, J. M. *Mental development in the child and the race*. New York: Macmillan, 1900.

— *Thought & things or genetic logic. Vol. I. Functional logic*. New York: Macmillan, 1906.

— *Thought & things or genetic logic. Vol. II. Experimental logic*. New York: Macmillan, 1908.

— *Thought & things or genetic logic. Vol. III. Real logic, Interest & art*. New York: Macmillan, 1911.

BARBER, B. *Science and the social order*. Glencoe, Illinois: the Free Press, 1952.

BEARDSLEY, D. C., & WERTHEIMER, M. (Eds.) *Readings in perception*. New York: Van Nostrand, 1958.

BERGMANN, G. *Philosophy of science*. Madison: Univ. Wisconsin Press, 1957.

— & SPENCE, K. W. Psychophysical measurement. *Psychol. Rev.* 1944, 51, 1–24.

BERTALANFFY, L. von. An essay on the relativity of categories. *Phil. Sci.,* 1955, 22, 243–263.

BETH, W. E., MAYS, W., & PIAGET, J. *Épistémologie génétique et recherche psychologique.* (Études d'Épistémologie Génétique, I.) Paris: Presses Universitaires de France, 1957.

BROWN, G. B. *Science, its method and its philosophy.* New York: Norton, 1950.

BRUNER, J. S., GOODNOW, J. J., & AUSTIN, G. A. *A study of thinking.* New York: Wiley, 1956.

BRUNSWIK, E. *The conceptual framework of psychology.* Int. Encyc. Unif. Sci. Vol. 1, No. 10. Chicago: Univer. Chicago Press, 1952.

  – *Perception and the representative design of psychological experiments.* Berkeley: Univer. California Press, 1956.

  – Scope and aspects of the cognitive problem. In J. S. Bruner et al., *Contemporary approaches to cognition.* Cambridge: Harvard Univ. Press, 1957.

CAMPBELL, D. T. Operational delineation of "what is learned" via the transposition experiment. *Psychol. Rev.,* 1954, 61, 167–174.

  – Adaptive behavior from random response. *Behavioral Sci.,* 1956, 1, 105–110. (a)

  – Perception as substitute trial and error. *Psychol. Rev.,* 1956, 63, 330–342. (b)

  – *Leadership and its effect upon the group.* Columbus, Ohio: Ohio State Univ., Bureau of Business Research Monograph No. 83, 1956. (c)

  – Factors relevant to the validity of experiments in social settings. *Psychol. Bull.,* 1957, 54, 297–312.

  – Common fate, similarity, and other indices of the status of aggregates of persons as social entities. *Behavioral Sci.,* 1958, 3, 14–25.

  – Social attitudes and other acquired behavioral dispositions. In S. Koch (Ed.) *Psychology: a study of a science. Vol. 6. Investigations of man as socius: Their place in psychology and the social sciences.* New York: McGraw Hill, 1960, in press. (Duplicated draft distributed in 1957.)

  – & FISKE, D. W. Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychol. Bull.,* 1959, 56, 81–105.

CANNON, W. B. *The way of an investigator.* New York: Norton, 1945.

CRAIK, K. J. W. *The nature of explanation.* Cambridge: Cambridge Univer. Press, 1943.

CRONBACH, L. J., & MEEHL, P. E. Construct validity in psychological tests. *Psychol. Bull.,* 1955, 52, 281–302.

FEIGL, H. Existential hypotheses: realistic versus phenomenalistic interpretations. *Phil. Sci.,* 1950, 17, 35–62.

  – Some major issues and developments in the philosophy of science of logical empiricism. In H. Feigl & M. Scriven (Eds.) *The foundations of science and the concepts of psychology and psychoanalysis.* Vol. 1 of Minnesota studies in the philosophy of science. Minneapolis: Univer. Minnesota Press, 1956.

  – The "mental" and the "physical". In H. Feigl, M. Scriven, and G. Maxwell, (Eds.) *Concepts, theories, and the mind-body problem.* Vol. II of Minnesota studies in the philosophy of science. Minneapolis: Univer. Minnesota Press, 1958.

GARNER, W. R. Context effects and the validity of loudness scales. *J. exp. Psychol.,* 1954, 48, 218–224.

  – HAKE, H. W., & ERIKSEN, C. W. Operationism and the concept of perception. *Psychol. Rev.,* 1956, 63, 149–159.

GEORGE, W. H. *The scientist in action: a scientific study of his methods.* London: Williams & Norgate, 1936.

HERBERT, M. J., & HARSH, C. M. Observational learning by cats. *J. comp. Psychol.,* 1944, 37, 81–95.

HÖFFDING, H. *A history of modern philosophy.* Vol. II. London: Macmillan, 1900. (Trans. by B. E. Meyer).

HOVLAND, C. I. A "communication analysis" of concept learning. *Psychol. Rev.* 1952, 59, 461–472.

JENNINGS, H. S. *The behavior of the lower organisms.* New York: Columbia Univ. Press, 1906.

– *The biological basis of human nature.* New York: Norton, 1930.

KÖHLER, W. *The place of value in a world of facts.* New York: Liveright, 1938.

LORENZ, K. Die angeborenen Formen möglicher Erfahrung. *Z. Tierpsychol.,* 1943, 5, 235–409.

– The rule of Gestalt perception in animal and human behavior. In L. L. Whyte, (Ed.) *Aspects of form.* New York: Pellegrim and Cudahy, 1951.

LOVEJOY, A. O. *The revolt against dualism.* Chicago: Open Court, 1930.

MARGENAU, H. *The nature of physical reality.* New York: McGraw-Hill, 1950.

MAYS, W. The epistemology of Professor Piaget. *Proc. Aristotelian Soc.* 1954, New Series 54, 50–76.

MELØE, J. Dialogue on the hypothetical character of logical analysis. *Inquiry,* 1958, 1, 72–84.

MERTON, R. K. *Social theory and social structure.* Glencoe: Free Press, 1949.

– Priorities in scientific discovery: a chapter in the sociology of science. *Amer. Sociol. Rev.,* 1957, 22, 635–659.

MEYER, M. F. *Psychology of the other one.* Columbia: Missouri Book Co., 1921.

MICHOTTE, A. E. *La perception de la causalité.* Louvain: Inst. sup. de Philosophe, Études Psychol. Vol. VI, 1946.

NAESS, A. *Notes on the foundation of psychology as a science. Filosofiske Problemer* Nr. 9, 1948, Oslo University Press.

NESS, A. *Erkenntnis und wissenschaftliches Verhalten. Oslo:* I Kommisjon hos J. Dybwad, 1936. – "Ness" is a German synonym for the English "Naess".

NEWELL, A., SHAW, J. C., & SIMON, H. A. Elements of a theory of human problem solving. *Psychol. Rev.,* 1958, 65, 151–166.

NORTHROP, F. S. C. *The logic of the sciences and the humanities.* New York: Macmillan, 1949.

OPPENHEIMER, R. Analogy in science. *Amer. Psychol.,* 1956, 11, 127–135.

PEPPER, S. C., *World hypotheses.* Berkeley: Univer. California Press, 1942.

– *The sources of value.* Berkeley: Univer. California Press, 1958.

PIAGET, J. *Introduction à l'épistémologie génétique. I. La pensée mathematique, II. La pensée physique.,* and III. *La pensée biologique, la pensée psychologique, et la pensée sociologique.* Paris: Presses Universitaires de France, 1950.

– The child and modern physics. *Sci. Amer.,* 1957, 196, 46–51.

PLATT, J. Amplification aspects of biological response and mental activity. *Amer. Sci.,* 1956, 44, 181–197.

POLYA, G. *How to solve it.* Princeton: Princeton Univer. Press, 1945.

– *Mathematics and plausible reasoning.* Vol. I. *Induction and analogy in mathematics.* Vol. II. *Patterns of plausible inference.* Princeton: Princeton Univer. Press, 1954.

PRINGLE, J. W. S. On the parallel between learning and evolution. *Behaviour*, 1951, 3, 175–215.

PUMPHREY, R. J. Hearing. *In Symposia of the Society for Experimental Biology. IV. Physiological mechanisms in animal behavior*. New York: Academic Press, 1950.

QUINE, W. VAN O. *Mathematical logic*. Cambridge: Harvard Univer. Press, 1947.

RITCHIE, A. D. *Scientific method*. London: K. Paul, Trench, Trubner, 1923.

SCHRÖDINGER, E. *What is life?* New York: Macmillan, 1945.

SEGALL, M. H. Les pensées de Piaget (book review) *Contemp. Psychol.*, 1957, 2, 309–311.

SLUCKIN, W. *Minds and machines*. Harmondsworth: Penguin Books (A pelican book) 1954.

SMYTHIES, J. R. *Analysis of perception*. London: Routledge & Kegan Paul, 1956.

SPENCER, H. *Principles of psychology*. Vol. I. Part III, General synthesis. New York: Appleton, 1896.

SPIEGELMAN, S. Differentiation as the controlled production of unique enzymatic patterns. In *Symposia of the Society for Experimental Biology, II. Growth in relation to differentiation and morphogenesis*. New York: Academic Press, 1948.

TOLMAN, E. C. *Purposive behavior in animals and men*. New York: Appleton-Century, 1932.

 – & KRECHEVSKY, I. Means-ends-readiness and "hypotheses". *Psychol. Rev.*, 1933, 40, 60–70.

UEXKÜLL, J. von. *Umwelt und Innenwelt der Tiere*. Berlin: Springer, 1920.

WALTER, W. G. *The living brain*. New York: Norton, 1953.

WHITROW, G. J. Why physical space has three dimensions. *Brit. J. Phil. Sci.*, 1955, 6, 13–31.

WHORF, B. L. *Language, thought, and reality*. Cambridge: Technology Press of Massachusetts Institute of Technology, 1956.

WOODWORTH, R. S. *Experimental psychology*. New York: Holt, 1938.

ON WORTHWHILE HYPOTHESES

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Veracity, equity, propriety — they are all capital virtues. But not the only virtues in the world. All the same, we often act as if they were. We sometimes become so obsessed by the excellent idea to tell nothing but the truth, that, to be on the safe side, we tell nothing. Thus the path of rectitude turns to a highway of platitudes.

Consider the following sentence, T_0: *A normal man does not exist.* T_0 occurs in a textbook in psychology. It sounds singular at first sight. Staggering, one might even say. If we look up the word "normal" in Oxford, Webster[2], Wyld or Roget[3], we find such suggestions for synonymic alternatives as, *e.g.:* "regular", "ordinary", "usual", "most frequently occurring", and the like. So there is this psychologist who has the audacity to maintain that the most frequently occurring, regular, ordinary, usual human being, does not exist at all! It certainly seems to be the kind of sentence that deserves a lifted eyebrow. And the empirical semanticist · (S) rushes to the psychologist (P) for an interview.

> S: Sir, may we ask you what you mean by the sentence: "A normal man does not exist?"
> P: I mean what I said.
> S: Oh, I see. That most men do not exist?
> P: That is of course not what I mean. I am merely pointing to a fact, based on life-long experiences, that all human beings are more or less psychologically abnormal. Neurotic, if you see what I mean.
> S: Everybody?
> P: Everybody.

S: You yourself, sir?

P: Humph. Eh. Well, I am a human being, am I not? And similarly constituted . . .

S: Oh, well. No rule without exceptions, you know, the exception that proves the rule and that sort of thing.

P: No. This rule is absolutely without exceptions. That's the way it's bound to be. Let me explain: All placental mammals are viviparous.

S: With a few exceptions.

P: Consequently, they are all born. And, at a certain stage, disconnected from their mother's body. Now: As we psychologists define "traumatic experience" this umbilical disconnection necessarily entails a traumatic experience to the infant. "Neurosis", on the other hand, is nothing but a name for the functional manifestation of such a traumatic experience.

   Consequently: a) All mammals are neurotic. b) All human beings are mammals. c) All human beings are neurotic. Quod erat demonstrandum. Simple logic, isn't it?

S: Quite. What now puzzles me is only this: Why would you want to use "neurotic" in such a way that everybody, including other mammals as well, becomes neurotic in this sense of "neurotic"?

P: That is exactly what I just explained. I am merely pointing to a fact, based on life long experiences, that all human beings are more or less psychologically abnormal. Neurotic, if you see what I mean.
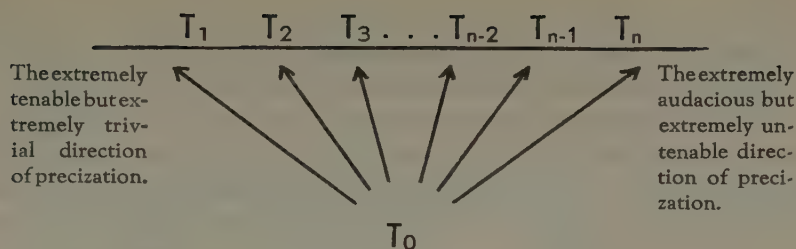
S: Everybody?

P: Everybody.
   Etc., etc., etc., *ad nauseam*.

In the present article a terminology will be proposed in which to distinguish significant and insignificant hypotheses.[1] According to this terminology the verbal behaviour of the psychologist in the above example may be described as an oscillation between an extremely *audacious* and an extremely *trivial* hypothesis. He is torn between two decisions. On the one hand, he wants to be veracious and tell the truth; on the other, he wants to make a worthwhile statement, *viz.* present a hypothesis which seems to have some audacity *as well as* tenability. But, as the saying goes: "The new things aren't true things, and the true things aren't new things." So what is to be done? The situation calls for a sentence, T_0, which admits of a sufficiently wide range of precization T_1, T_2, T_3, ... T_n to bridge the gulf fixed between a) an extremely tenable, but trivial hypothesis as transmitted, say, by a sentence, T_1, and b) a highly audacious, but untenable hypothesis as indicated by a sentence, T_n.

$$T_1 \quad T_2 \quad T_3 \ldots T_{n-2} \quad T_{n-1} \quad T_n$$

The extremely tenable but extremely trivial direction of precization.

The extremely audacious but extremely untenable direction of precization.

$$T_0$$

The highly ambiguous T_0 borrows, so to speak, audacity from one direction of precization (T_n), and tenability from the other (T_1). T_n may tentatively be formulated as follows. T_n *There are no members at all in the class of human beings to which most human being belong, viz. normal, usual, most frequently occurring human beings.* On the other hand, the extremely trivial direction of precization could, in order to bring out the contrast, be phrased thus: T_1 *If the expression "normal" is used in such a way that nothing can be normal and at the same time belong to the class of human beings, then no human being will ever be normal in this (peculiar) sense of "normal".* An intermediate hypothesis, expressing a moderate degree of both tenability and audacity, may be exemplified by T_m. T_m *No living person today is justified in claiming that he is completely normal in the sense that his cognitive or emotional processes use nerves insufficiently disrupted so as to interfere with a 100 % effective life adjustment.*[4]

Factual Triviality and Tenability

It is a regrettable truism that extremely tenable, "absolutely true" hypotheses are proportionally trivial and completely devoid of "autotelic" interest — as aptly illustrated by the present sentence itself. "Extremely trivial" applies in this connection to any hypothesis the truth of which is known to be unquestioned within a given reference of communication, R_a. The more universal the reference of communication, the more universal is correspondingly the truth and triviality of the hypothesis concerned. Examples of an overly *universal* triviality are furnished by hypotheses the truth of which is claimed to be absolutely unquestionable within any conceivable reference of communication, — "true under every possible state descriptions," "cannot possibly be false," "true in all possible worlds" etc. The other end of the continuum from universal to more *special* triviality is formed by hypotheses the unquestioned truth of which is strictly limited to a reference of communication consisting

solely of one singular communicational event, — "true only for one particular person, P_1, in one particular situation, S_1."[5]

It goes without saying that a hypothesis found to be invariably *non-controversial* within a certain reference of communication, R_a, will never be propounded within R_a, except in order to form a basis or a platform on which R_a-communicators can stand and debate the relative tenability of other, genuinely *controversial* hypotheses within R_a. A non-controversial hypothesis thus employed as an argumentative point of departure for determining the relative tenability of controversial hypotheses within a given reference of communication, R_a, we will call "*a descriptive supposal*", S, or, in short, a "datum" in R_a[6]. The following sentence, [1]S, will presumably transmit a hypothesis which might come in useful as a datum in a reference of communication consisting of almost every competent mathematician within the span of years from *ca.* 1824 to at least 1959: [1]S *It is impossible to solve the general quintic equation by means of radicals*. However, [1]S would hardly do as a datum within an extended reference of communication, *e.g.* including mathematically incompetent communicators or mathematicians before 1824 or after 1959. [1]S would, within such an extended reference of communication, change from a descriptive supposal, a datum, [1]S, to a more or less controversial hypothesis, T_D. And one should have to look for other, "more universally accepted hypotheses", "more basic data", by means of which to determine the relative tenability of [1]T as compared to the tenability of competing, mutually exclusive, alternative hypotheses, [2]T, [3]T... etc. Below are a few examples of such fairly basic descriptive supposals: [a]S *If Peter and Dick are outside in the hallway then Dick is outside in the hallway.* [b]S *Almost all human beings have at least one leg.* [c]S $\sim \times 0 = 0$. [d]S *1960 is a leap-year*. But hardly any of the cited data (with a possible exception of [c]S) would be of any help in an attempt to determine the relative tenability of the above hypotheses: [1]T, [2]T, [3]T ... etc. They would, for this purpose, be completely devoid of discriminatory powers.

Take on the other hand another example. Let us say that within a certain reference of communication we have two controversial hypotheses: [1]T *It will be sunny tomorrow* and [2]T *It will be rainy tomorrow*. Let us furthermore assume the following available data: [1]S *The weatherman says: "It will be sunny tomorrow."* [2]S *Our family barometer is on "storm"*. In this case one might quite convincingly show that the relative tenability of [1]T and [2]T is dependent upon a) the tenability of [1]S and [2]S and b) the tenability of the following four implicational hypotheses:

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$^1T^1$: *If* ^{1}S *is tenable then* ^{1}T *is tenable.*
$^1T^2$: *If* ^{2}S *is tenable then* ^{1}T *is tenable.*
$^2T^1$: *If* ^{1}S *is tenable then* ^{2}T *is tenable.*
$^2T^2$: *If* ^{2}S *is tenable then* ^{2}T *is tenable.*

The first problem is essentially solved by the very fact that ^{1}S and ^{2}S are intended as data within the assumed reference of communication. $^1T^2$ and $^2T^1$, on the other hand, seem to be equally untenable. This leaves us with the task to determine $^1T^1$ and $^2T^2$. In other words: how reliable a clue is the weatherman's prediction as compared to the barometer reading? a) If this question is taken seriously, *i.e.* meticulously scrutinized in order to secure an objective answer,[7] and b) provided ^{1}T and ^{2}T are the only hypothesis alternatives to constitute the choice situation and ^{1}S and ^{2}S are the only relevant and available data, *then* we will say that the relative, *actual* tenability of ^{1}T and ^{2}T has been determined — within the indicated reference of communication, R_a. Let us furthermore assume that ^{1}T is finally found to be more tenable in R_a than is ^{2}T. This result may well come as a surprise to the communicators in R_a. Up to now they had, perhaps, rested assured that ^{2}T's tenability would greatly surpass ^{1}T's. In that case we will say that ^{2}T had a higher degree of *primafacie* tenability in R_a than had ^{1}T, untill ^{1}T's higher degree of *actual* tenability was established.

Factual Audacity

In general a hypothesis as fairly adequately transmitted, say, by ^{1}T is here called "more audacious" and "untrivial" than is another hypothesis as objectively transmitted, *e.g.* by ^{2}T within a reference of communication, R_a, if and only if a hypothesis transmitted by ^{2}T has a higher degree of *primafacie* tenability than has the hypothesis most plausibly transmitted by ^{1}T. And by "degree of *prima-facie* tenability" is here to be understood: "the degree to which an objective outline of arguments put forward within R_a *for and against* a hypothesis shows an overweight of pro-arguments.."[8] Thus T_0 "*There are no normal human beings,*" would undoubtedly show a conspicuous overweight of pro-arguments when interpreted in the direction of T_n, whereas surely the scale would tip over to the con-side if T_1 were used as a transmitter alternative. What exactly would happen if T_m had been chosen is not easy to predict. A communicational reference exclusively consisting of psychoanalyst-communicators would, most likely, tend to favour the pro's. In a more normal reference of communication, *i.e.* more repre-

sentative for the total population of a whole country, the opposite result would be more probable. The highest degree of factual triviality within a reference of communication, R_a, is reached by hypotheses functioning as descriptive supposals or *data* in R_a. The highest degree of actual audacity within R_a manifests itself in the negation of R_a's data. It goes without saying that what is considered data within one reference of communication, R_a, may well be highly questionable or controversial hypotheses within another reference of communcation, R_b — and vice versa.

Hence in order to determine the tenability of a hypothesis within an extended reference of communication, consisting *e.g.* of both R_a and R_b, it is imperative that the members of R_a and R_b are capable of questioning their specific data and of turning them into controversial hypotheses if necessary. Thus there will be no limit as to how universal we can make our reference of communication. It is only a question of how far we want to go in the process of turning data into controversial hypothesis by employing so-called "more basic", "more universal" data. We shall regard 'factual audacity' or 'factual triviality' respectively as the more important concept of 'audacity' and 'triviality'. However, two other concepts deserve to be mentioned.

Practical Audacity

Consider a hypothesis as transmitted, quite objectively, by ${}^aT$: *The interior of Sirius is filled with mock-turtle*. It is hard to imagine any reference of communication within which ${}^aT$ would not show a relatively high degree of audacity.[9] On the other hand, consider a hypothesis as expressed by ${}^bT$: *A third world war will break out within two weeks*. In terms of *prima-facie* tenability this hypothesis, ${}^bT$, is far from being as audacious as ${}^aT$. Yet ${}^bT$ will greatly surpass ${}^aT$ in making a hullabaloo within most communicational references (including the astronomical). More lives will be more changed due to ${}^bT$ than to ${}^aT$. We want a name for this quality. The name suggested here is "practical audacity" ("practical triviality" respectively). However, if nothing else is explicitly stated, "audacity" in this article refers consistently to "factual audacity". The concept of 'factual audacity' is far more essential for purposes of objectivity in any kind of argumentative discourse. 'Practical audacity' enters seriously into the picture only when the two concepts of 'audacity' coincide. As the case may be when the hypotheses in question are

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without any immediate applicability to what is commonly called "the practical life".

This may be aptly illustrated by the so-called "discoveries" within the field of mathematics. Countless, of course, are the mathematical hypotheses which may be propounded merely by combining entities, commonly accepted within a reference of communication consisting of any conceivable terrestrial language society. These are mathematical hypotheses as advanced by the cashiers in the super-markets whenever they ring up a sale on the cash register. Some of these hypotheses, usually additions, may possibly not have occurred before in the history of mathematics. And yet they are considered factually trivial and are rarely questioned.[10] They are not "mathematical discoveries". That is to say, they would be absolutely devoid of theoretical interest within a communicational reference of competent mathematicians, R_{mat}. On the other hand, any audacious hypothesis, questioning or negating any data within R_{mat}, will *ipso facto* change the "practical life" of all the R_{mat}-communicators, *i.e.* in their capacity of mathematicians.

This point brings up another, even less convincing audacity/triviality distinction, to wit, the distinction between factual audacity and what one might call "formal audacity".

Formal Audacity

Consider an example where a student, P_1, bets another student, P_2, ten dollars that the following hypothesis, ^0T, is tenable: *Dick or Jack is outside in the hall.* Let us say that we explain to P_1 and P_2 the ambiguity involved in the expression "or", and ask them to select one of the two precizations of "or" (a_0) to be employed in the sentence ^0T *Dick or Jack is outside in the hall,* either a_1 *and/or* (veljunction) or a_2 *either/or* (autjunction). The choice is easily foreseen. P_1 would undoubtedly suggest (a_1), whereas P_2 may prefer (a_2). And understandably so. We have here, it has been maintained, in principle four possibilities: a) *Dick and Jack are both in the hall,* b) *Dick, but not Jack, is in the hall,* c) *Jack, but not Dick, is in the hall,* d) *Neither Dick nor Jack is in the hall.* If P_1's suggestion for usage of "or" (a_1) is accepted, P_1 could collect his money in three of these four formal possibilities, *viz.:* a, b and c. Whereas a usage of "or" in the direction of a_2 would change the formal score to a fifty-fifty bet. Thus we may distinguish two hypotheses: ^1T and ^2T. ^1T *Either Dick or Jack is out in the hall or both Dick and Jack are out in the hall,* ^2T *Either Dick or*

Jack is out in the hall but not both of them are out in the hall. ¹T is false in only one of four formal possibilities. ²T is false in two of four formal possibilities. In that case ²T would be said to have *a higher degree of formal audacity* than has ¹T. (¹T has a higher degree of formal triviality than has ²T.) A hypothesis with an even higher degree of formal audacity than ²T may be indicated by ³T *Both Dick and Jack are out in the hall.* ³T would be false in three of four formal possibilities (b, c, d).

However, when P_1 rushes towards the door, in order to confirm or disconfirm ⁰T he and P_2 at once realize that Dick and Jack are sitting at the table and have been there all the time! Or it suddenly occurs to them that Jack and Dick were killed in a traffic accident years ago. It sounds somehow silly then to persist that still . . . formally . . . logically . . . in principle . . . ³T *is* more audacious than ²T, and ²T *is* more audacious than ¹T . . . etc. . . . . These considerations apply to all kinds of formal probability hypotheses.[11] It is preposterous to maintain that a hypothesis: "*I will now throw a 4 or 5 with this die*" is formally less audacious than: "*I will now throw a 1 with this die*" in a context where the die in question has no pips at all. Formal audacity can only add a trifle to the factual audacity under rather special circumstances. For example: ᵉT *There is at least one lady with two heads*, is not as audacious as ᶠT *There are no ladies with less than two heads.* If ᵉT has any factual audacity, then ᶠT has even more of it, thanks to its higher degree of formal audacity. However, if ᵉT and ᶠT were rephrased so as to transmit more trivial hypotheses, for instance by replacing "one head" for "two heads" in the above examples, then there would be no noticeable differences in factual audacity between the two hypotheses.

Formal Triviality

Let us return to the students P_1 and P_2. Throughout the centuries an unaccountable interest has been attached to hypotheses with an extreme degree of formal audacity and to hypotheses with a corresponding level of formal triviality. The maximum of formal triviality in our example may be expressed by ⁰T *Either Jack or Dick or neither Jack nor Dick is in the hall, or both Jack and Dick are in the hall.* ¹T would be false in *none* of the four formal possibilities (a, b, c, d). The same holds true for cognitively identical transmitter alternatives of ⁰T *e.g.* ⁰T₁ *If Dick and/or Jack are/is in the hall, then Dick and/or Jack are/is in the hall.*[12] The other extreme, the *maximum* degree of audacity is aptly

conveyed, *e.g.* by ⁴T *Both Jack and Dick are in the hall and neither Jack nor Dick is in the hall* — or, of course, by any possible negation of ⁰T or its synonyms. Thus we get a continuum of hypotheses ranging from a maximum degree of formal audacity transmitted by ⁴T down to a minimum of formal audacity as expressed by ⁰T.

a) In ⁴T all of the four formal possibilities are false,
b) In ³T three of the four formal possibilities are false,
c) In ²T two of the four formal possibilities are false,
d) In ¹T one of the four formal possibilities is false and
e) In ⁰T none of the four formal possibilities is false.

Philosophers have been completely enchanted by the extremes of these possibilities. The ⁰T type of hypothesis seems to provide an "unshakable, absolute truth," unknown in any other kind of hypothesis. Consequently, so it seemed, if we could only confine our ratiocination and deliberation to hypotheses with this extreme degree of formal triviality, then we could finally rest in an eternal truth. And this is exactly what appears to be, or has been claimed to be the case within so called "deductive systems" of hypotheses.[13] Every singular sentence within such a system transmits a formally trivial ⁰T-type of hypothesis, which is absolutely true in the same ⁰T-sense of "absolutely true". That is to say, the singular sentences in the system are thus connected with each other, for instance by *implicational hypotheses* as those previously described or otherwise, so that the whole system may be regarded as conveying one enormous, formally totally trivial hypothesis. However, if this also implies that all the singular sentences within such an enormous deductive system are transmitting messages as fascinating as the one conveyed by ⁰T above, then it may be hard to understand what *motivates* anybody to construct deductive systems. But they are not that trivial. A somewhat elaborate deductive system, for instance a mathematical or symbolic logical calculus, possesses a complexity which deprives every singular hypothesis within the system of its formal or factual triviality. In Grand Palais de la Science in Paris, there is a circular room where the walls are decorated with a seemingly endless spiral of numbers: 3.14159265 ..., the so-called "Ludolphian number", pi, computed with thousands of decimals. It is probably not too much to say that so far, nobody has, when confronted with this singular mathematical hypothesis, felt the same kind of *triviality* as *vis à vis* the

hypothesis transmitted by the following sentence: "If it is true that Jack and Dick are outside in the hall, then it is also true that Dick is outside in the hall."

So let it be admitted, then, that singular hypotheses within a complex deductive system are neither formally nor factually trivial. But are they tenable? One of the major purposes in constructing complex deductive systems was the gathering together of extremely tenable or "absolutely, unshakably true" hypotheses.

Let us say that we advance the more or less sensational hypothesis, ^1T *Nine thousand, three hundred and sixty-six to the three thousand and forty-second power equals approximately thirty-three thousand billion billions.* True or false? If we decide to conceive of mathematics as a deductive system, then the answer is clear. Either ^1T is in accordance with the rules of the system, or ^1T is not in accordance with the rules of the system. In the former case, ^1T is true, in the latter, false. Then how does one go about ascertaining whether or not ^1T is in accordance with the rules? First of all, one would have to inquire into these *rules*. In other words, which are the most advisable proposals or the most tenable hypotheses these days, concerning the so called "formal" relationships between singular sentences in the complex deductive system concerned. Next comes a thorough investigation of ^1T's accordance with these rules as compared to possible, competing, alternative hypotheses: ^2T, ^3T, ^4T... ^{n-1}T, ^nT. On account of our human limitations, we may here prefer to make use of certain handy instrumental devices, such as as electronic digital computers. To be on the safe side, we consult not only one, but several of these machines, and the best that money can buy. Furthermore, let us assume that none of the computers ends up with any suggestions in the neighborhood of ^1T, whereas two of them come close to hypothesis ^6T. And there are indications available to the effect that the third employed computer (the one that supported hypothesis ^{111}T) was out of order. In that case, we will say that our investigation has brought forth some fairly convincing arguments for ^6T and against (among others) our own hypothesis ^1T.

In short, the "absolute, unshakable truth" seems to have disappeared along with the extreme, formal triviality. The deductive hypotheses do not seem to differ from any other kind of hypotheses. Either they are apparently quite true and trivial, *or* they possess *some* audacity and a corresponding degree of uncertainty.

Moreover, we see no need for any fundamental distinction between the two following kinds of hypotheses: a) hypotheses, the tenability of

which (or: proposals, the advisability of which) is determined by references to empirical data[14] or by any other spatio-temporal references, — and b) hypotheses, the tenability of which (or: proposals, the advisability of which) is not or, allegedly, *cannot* be determined by empirical, observational, or other spatio-temporal data.[15]

True enough, we sometimes get the impression that we solve a problem by so called "pure reasoning". For instance: Two girls look so similar to me that I infer: "you must be twins." "No," they reply, "that is not true. We were born at the same time, and the same place of the same parents, but we are not twins." While I brood on this problem, the door opens, and in comes a *third* girl, exactly like the others. "Oh, now I see what you mean: you are not twins; you are triplets." This hypothesis: "The two girls are not twins, but two of (three) triplets," is apparently a hypothesis, the tenability of which refers to empirical observations in time and space, i.e. of the three girls. But what if the third girl had not turned up in person? I just *saw* her ... as a negative or positive after-image, or the most vividly detailed, clear and colorful eidetic-image.[16] Or, I may not have seen her quite that clearly, vividly, detailed and in colour. More like, say, the mental pictures that chess players and mathematicians seem to have, and which are so convincingly described by Albert Einstein (as a representative of the mathematicians.)[17] Or the visual or other perceptual elements may be even less predominant. Even so, where do we draw the line ... ?

It seems that up to now all suggestions for distinguishing empirical, synthetic, etc. and various sorts of nonempirical, analytic, etc. hypotheses, are made more or less haphazardly, and have caused more confusion than enlightenment.[18]

We have conscientiously refrained, in this article, from any temptation to draw such a line. We have, here as elsewhere, contented outselves with a continuum. Besides, a continuum has, from the point of view of objectivity, the inestimable advantage that it deprives one of the absolutism and rigidity so often caused by clear-cut classifications.

The importance of discussing the imaginary extremes of formal triviality is the fatal propensity among communicators to drift by insensible degrees from a formally and factually audacious hypothesis into a totally unqualified triviality. As in the illustrative example with the psychologist who argued so vigorously that "no man is normal". He came in like a lion and went out like a lamb. That is to say: He started out with a sentence which sounded as if it were to transmit a hypothesis of extreme audacity — factually or even formally! But hard pressed at

the audacity end, he tiptoes over to the other tenability-triviality extreme, unperceived, especially by himself.

An extremely trivial hypothesis may serve as an excellent *datum* in an argumentation list where the purpose is to try to obtain agreement within a large and heterogeneous reference of communication. But nobody would even *want* to transmit to himself or any other receiver a *hypothesis* of an extreme degree of triviality. Consequently, a suggestion for precizations of a sentence (apparently intended to express a hypothesis) in the direction of an extreme degree of triviality, should, by and large, be rejected as *implausible*. And it is the more implausible the more conspicuously trivial the transmitter of the hypothesis appears to be.[19] Hence the sentence: "In Berkeley, it is either raining or it is not raining" is apparently intended to transmit — in a somewhat witty way — a message which may be less wittily transmitted as follows: "In Berkeley, there is either glorious sunshine, or the rain is pouring down." The thusly transmitted hypothesis is obviously not trivial. It is false whenever the weather in Berkeley is such that it does not permit a classification in either (or both?) of the two mentioned categories. In other words, the more conspicuously trivial a sentence is, the less watchful we have to be *vis à vis* our own inclinations to use an extremely trivial synonymic alternative as a cavern in which to seek shelter, when the more audacious synonymic alternatives turn out to be untenable.[20]

And vice versa for the extremes of formal *audacity:* the more conspicuously contradictory a sentence is phrased, the more likely it is that the sender has deliberately chosen the formulation — for wittiness or similar purposes! "Nothing is more sensational than a good platitude." (Oscar Wilde).

To Sum Up

A hypothesis transmissible by ^{1}T is more audacious than another statement transmitted by ^{2}T, if, and only if, ^{2}T has a higher degree of *prima facie* tenability within a reference of communication, R_a, than has ^{1}T. But should we infer from this that ^{1}T is also more *worthwhile* than ^{2}T? Certainly not. A hypothesis, as expressed by ^{1}T "*The interior of Sirius is filled with mock-turtle*" has certainly a considerably lower degree of *prima facie* tenability than most conceivable alternative hypotheses concerning the interior of Sirius. However, ^{1}T will most likely be inferior to the competing hypothesis with regard to *actual* tenability as well. Thus, a hypothesis expressed by a sentence ^{1}T would only be more

worth while, or more significant than another, expressed by ^{2}T, if the ^{1}T statement also turns out to have approximately the same degree, or a higher degree, of *actual* tenability (respectively *actual* advisability) than has the ^{2}T hypothesis. This is to say, a hypothesis ^{1}T is *more significant* than another hypothesis ^{2}T if and only if the estimated product of ^{1}T's audacity and ^{1}T's actual tenability exceeds the estimated product of ^{2}T's audacity and actual tenability.[21] Moreover, 'insignificance' may be presented in such a way that the product of: a) high degree of triviality and b) high degree of either untenability or inadvisability, would manifest a high degree of insignificance, as illustrated by the set of continua below:
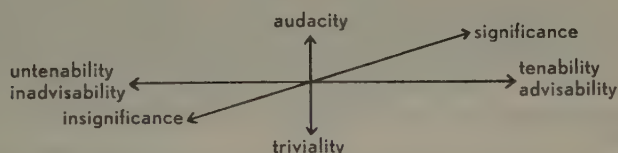
The threat of insignificance is especially imminent within relatively new and "soft"sciences. A relatively newly established science — as for instance the science of communication with which we are dealing in the present article — will have to, or feels called upon to, develop a system of concepts, symbols, distinctions, phrases and expressions hitherto unheard of. The actual benefits of such systems of symbols, new terminologies and "theoretical frameworks" are easily overrated.[22] The following accomplishments are, two by two, most frequently confused: a_1) We have advanced a tenable hypothesis, a_2) We have been able to apply the suggested terminology to a concrete case, without committing too many apparent absurdities, contradictions or tautologies. Or: b_1) We have contributed to the world of science a new, exciting, sensational hypothesis, b_2) We have succeeded in couching an old-fashioned, commonplace, pedestrian platitude in such new, exciting, sensational words that it sounds: ... new ... exciting ... sensational.

[1] *"Hypothesis"* is here not used in accordance with any of the more ordinary language usages, but in an extremely wide sense. If it makes sense to say about a statement that it is true or *tenable,* or false or *untenable,* then this statement is a hypothesis in our sense of "hypothesis". A statement which is not a hypothesis will be called a *"proposal".* A proposal is not true or *tenable,* or false or *untenable.* It is good or *advisable,* or bad or *inadvisable.* In fact the present article might just as well have been concerned with *worth while proposals.* It can still be read as such if the following substitutions are made. Read: *"proposal"* for "hypothesis", *"advisable"* for "tenable", *"proposal* alternatives" (^1T_n, ^2T_n ... etc.) for "hypothesis-alternatives", *"normative suppposals"* (^1S_n, ^2S_n ... etc.) for "descriptive proposals" (*"norms"* for "data") — that is all.

"Advisable proposal" and "tenable hypothesis" are, in our opinion, preferable to "normative statement" and "descriptive statement" respectively. The latter distinction is too easily associated with grammatical categories, and people with some scholarly background therefore often have an erroneous impression that the classification of a certain sentence as transmitting a normative or descriptive statement is more or less obvious, self-evident, like the classifications in verbs/nouns, imperatives/indicatives ... and so forth.

[2] *New International Dictionary of the English Language. Unabridged.* (Springfield, Mass. 1958.)

[3] *Roget's International Thesaurus of English Words and Phrases.* (New York, 1956.)

[4] This example was first used in an essay by the author of the present article to illustrate certain characteristics of a suggested continuum connecting the so-called "negative analytic" or "contradictory" statement at the one end with the "positive analytic" or "tautological" statement at the other, over a bridge of more or less "negative synthetic" and more or less "positive synthetic" statements. Cf. "Analytiske og syntetiske utsegner". *Norsk Allkunnebok,* Vol. I, Hefte 1, (Oslo, 1945.) A similar idea has been brilliantly suggested by Quine, W.O., in: *From a Logical Point of View,* (Harvard Press, 1953). Cf. my *Objectivity in Communication and Argumentation,* §§ 17.3 and 17.4 for further details.

[5] As a rule a communicator rarely indicates explicitly what in a given communicational event is supposed to be the intended reference of communication. This is, of course, the case regardless of the whether the communicator is communicating with or to *other* communicators (interpersonal communication) or with or to himself (intrapersonal communication). Most often the communicator will not be clear himself as to what reference of communication he more specifically had in mind.

[6] The procedure for constructing descriptive (and normative) argumentation maps is presented in detail in my *Objectivity in Verbal Communication,* Chap. V and in *Communication for Administrators,* pp. 1–21. (In normative maps we have: "proposal alternatives" and "normative suppposals" or "norms".)

[7] *E.g.* by a) mapping out a complete list of the relevant arguments for and against each implicational hypothesis and b) establishing a procedure which permits one to derive from this descriptive argumentation map a reasonable determination of the tenability of the implicational hypothesis concerned. (Cf. *Objectivity* ... Chap. V, etc.)

[8] For a more detailed description of what is meant by "overweight of proarguments in a for-and-against list of arguments," see *Objectivity,* Chap. V.

[9] As compared to competing, mutually exclusive alternative hypotheses (e.g., *Sirius' interior is filled with carbolic acid*).

[10] Cf. Poincaré, H.: "La Science et l'hypothèse" (1902), Part I and "Science et Méthode" (1908), Chap. III.

[11] *E.g.*, to what is designated "elementary statements of probability$_1$" (*i.e.*: "degree of confirmation of a hypothesis relative to given evidence") in Carnap, R.: "Logical Foundations of Probability" (Chicago, 1950).

[12] We should not be too certain, though, that ^0T and $^0T_1$ are cognitively identical. It may be that after careful consideration, we would find that the latter does not meet the required synonymity prerequisites. The fact that two transmitters both are interpreted to express a maximum degree of formal triviality, makes it psychologically difficult for us to imagine circumstances under which we would accept the one transmitted hypothesis as more tenable. This state of affairs may mislead us to believe that they both could replace each other in a context without any change in meaning of the intended message.

[13] Some sciences are frequently described as a (huge) deductive system, *e.g.* Mathematics, Logistics or Kinematics. (Note: the word "hypotheses" is used in the wide sense, proposed in this article. A commonly accepted terminology does not permit one to speak about hypotheses within deductive systems.)

[14] In the "descriptive supposal" sense of "data" as indicated in this article (or more detailed in *Objectivity*, etc.)

[15] This takes care of the following traditional distinctions as well:

Universalia ante rem	– Universalia post rem
Essential	– Accidental (existential)
Realism	– Nominalism
Analytic	– Synthetic
Rationalism	– Empiricism
Transcendens	– Immanens
Intentionality	– Causality
Questio Juris	– Questio facti
Das Nichtzusammenwahrseinkönnen	– Das Nichtzusammenglaubenkönnen
Ästhetik von oben	– Ästhetik von unten
Vérités de raison (nement)	– Vérités de fait

[16] On the borderline between perception and conception or idea ("Phänomene die eine Zwischenstellung zwischen Empfindung und Vorstellung einnehmen"). Cf. Jaensch, E.: "Die Eidetik und die typologische Forschungsmetoden," (Leipzig, 1933).

[17] Cf. Hadamard, I.: *Psychology of Invention in the Mathematical Field*, (Princeton, 1949.) (See also Poincaré, *ibid.*)

[18] However, recent investigations (at Univ. of Calif., Berkeley) show convincingly that any "normal" audience can be biased by a short (240 words) lecture on the analytic/synthetic-distinction, to perform the classification of almost any sentence in one of these two categories. They suddenly seem to lose all their commonsense insights in language usage. They sincerely claim that such sentences as: "Out West men are men", "A guy is a guy", "Rose is a rose is a rose", "Not all unmarried men are bachelors", "My mother was a real spinster", "The coffee in the coffee-shop is not coffee", cannot express anything but *either* positive analytic statements *or* negative analytic statements (contradictions.) Their attitude can most aptly be compared to

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an audience watching a detective mystery: all the commonsense, most probable solutions are disregarded *because* they are the commonsense, most probable. "It can't be that easy." Cf. a forthcoming article in this Journal: "What Should We Say?"

More generally the investigations seemed to indicate that such and similar "logico-maniacle" attitudes can be induced in an average respondent, simply by confronting him with an unrefined, straightforward questionnaire, concerning language usage. Thus these types of questionnaire do not seem to contribute noticeably to unravel "ordinary language usage". In fact they may be almost as distortive in this respect as discussions on "what should we say" within logic-of-language seminars.

[19] This point is convincingly advanced by Meløe, J. in "Dialogue on the hypothetical character of Logical Analysis", *Inquiry,* Vol. I, No. 1, p. 84.

[20] Meløe, J., *ibid.,* cites an example from Quine, W., *loc. cit.:* "No bachelor is married" which, according to Meløe, still admits of both trivial and audacious plausible synonymic alternatives whereas this is not so in the example: "No unmarried man is married" (*i.e.* does not admit of extremely trivial plausible interpretations).

[21] And vice versa for proposals: a proposal ^{1}T, is in this article *more significant* than another proposal ^{2}T if and only if the estimated product of ^{1}T's audacity and ^{1}T's actual advisability exceeds the estimated product of ^{2}T's audacity and ^{2}T's actual advisability.

[22] Cf., as an awful warning, Parsons, T.: *The Social System,* (Glencoe: The Free Press, 1951.)

WHO MEANS WHAT BY 'SYNONYMY'?

by

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I. "How are we to understand the attribution of 'is synonymous with' in a natural language?" This still fashionable question is ambiguous: it admits of two or more kinds of answer which it is well to distinguish. The question may intend: (A) "What methods, tests, or criteria, if any, are used by Mr. Z, an ordinary user of natural language *'L'*, when for some purpose having to do with his use of *L,* Z is trying to decide whether or not two terms of *L* are alike in meaning?" Or the question may intend: (B) "What methods, tests, or criteria can be employed by Professor P, a visiting philosopher, when for some purpose having to do with his description or analysis of *L,* P must decide whether or not two terms in *L* are alike in meaning?"

But this way of putting the second question is elliptical. Since *L* is an ordinary or natural language, a language used by Messrs. X, Y, and Z, what Professor P must decide is: (1) When the use by X, Y, and Z of two *L* terms is such that he, P, can call those terms "synonymous". But in order clearly and consistently to apply "synonymous" to some uses of *L,* Professor P will have had to decide: (2) Some criterion for his application of his analytical sense of "synonymous"; P must have some criterion according to which he will be able to discriminate, among all the uses of *L* by its users, particular uses of *L* terms which he can call instances of synonymy.

This logically prior question — 2 —in itself need present no problems: let us call P's descriptive or analytical meta-language "*LM*" and call synonymy, as he wishes to define and to use it in his analysis, "*LM*-synonymy". (We can call synonymy in Z's use of it "*L*-synonymy".) Now if P is to use *LM* in his analysis of the uses of *L,* and particularly if he wishes to discriminate and to understand instances of *L*-synonymy,

he will define "*LM*-synonymy" in terms of tests or procedures which he can actually employ in his examination of the various uses of *L*.

Ideally, one would suppose, P's tests for the application of "*LM*-synonymous" would be such that "*LM*-synonymous" applied to all and only to those relations of terms which Z called " *(L)* alike in meaning"; where some fairly systematic failure of coincidence appeared, P might decide to increase or to discriminate among his tests, and by reference to this refinement of his criteria, distingush *LM*-synonymy-1, *LM*-synonymy-2, and so forth; what seemed to be random and infrequent divergences could properly be put down to error, or perhaps to some unsystematically idiosyncratic use of "*L*-synonymy" by a particular subject. If it happened that there were some frequent and apparently regular, relatively systematic use by X, Y, and Z of "*L*-synonymous" which could not be discriminated by P's tests, or to which his criteria for "*LM*-synonymy" did not apply, it would seem most natural to put this down to some failure of P's analysis; it would seem rather odd to blame X, Y, and Z.

And yet one can imagine a situation wherein this odd accusation would appear to be made by Professor P, who at the same time says that he wishes to understand 'the attribution of "alike in meaning" in a natural language'. Imagine, for instance, that as used by the users of *L,* "*L*-synonymous" itself happened to be systematically ambiguous, that is, that there were different kinds of tests used by Mr Z to determine the applicability of "*L*-synonymous". It could be that Z had, say, three different kinds of criterion or tests which he at different times used to judge whether or not two terms in *L* 'meant the same'; it could also be that Z, seldom concerned to analyze his own linguistic pratice, often failed to distinguish his uses of these various tests, despite the fact that they might give differing results; "*L*-synonymous-1" might sometimes apply to different relations of terms than "*L*-synonymous-2", etc. Suppose now Professor P to approach this untidy situation, and suppose that his criterion for "*LM*-synonymous" were such that it applied to the results of no more than one of Z's kinds of test. If it were also the case that P's one meaning of "*(LM)* synonymy" seemed to him especially clear (perhaps by reference to some ideal of clarity far finer than any to be found in two-thirds of Z's practice), economical (sufficient perhaps for the analysis of some 'language' whose needs were fewer, and better understood), or even uniquely reputable (perhaps according to some antecedent standard of ontological propriety), then P might well refuse to content himself with the modest admission that he failed to under-

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stand the attribution of "*(L)* alike in meaning" in a natural language; P might go on to claim that all occasions on which Z said "*(L)* synonymous" were occasions on which Z did not understand what he himself was saying. The various parentheses in that labyrinthine sentence are supposed to suggest that their contained qualifications are seldom explicitly stated — a fact which may account for Z's impatience at being told that he is somehow not entitled to make a kind of judgment which he often finds it useful to make, or his annoyance when informed that he never understands what he is doing when he makes it.

However exaggerated these accusations may appear, even Z will admit that they have some justice. The user of ordinary language might well be willing to acknowledge a frequent failure to be clear about how he arrived at a judgment of likeness of meaning. He might even go so far as to admit less than perfect understanding of what exactly he meant in saying that two terms were *L*-synonymous. But he would probably not concede that such judgment was inevitably or even usually groundless, or that it marked no real distinctions in his own usage, or that he *could* not discriminate and make clear to himself — or even to the unsympathetic P — his various criteria for attributions of "*L*-synonymous".

The fact is, of course, that we all are somewhat P and considerably Z when our own natural language is in question. But even though we can play both roles — as Z using our language, as P describing, analyzing, and even criticizing that use — these roles can be more or less distinguished. The different character of these parts, and the consequent possibilities for misunderstanding would seem to suggest that when "we find ourselves asking whether two expressions in a natural language are synonymous",[1] or "how we establish synonymy even in a given situation",[2] it is advisable to ask "Who?" or "Which we?"[3]

II. In order to illustrate the misunderstandings that can arise when these differences in role are not made explicit, it might seem most useful actually to assume the role of Z, and safest to speak abstractly of The Problems of Professor P. It does seem safe to say that each of us can stand with confidence in the role of Z, since that is where most of us stand most of the time, but it would be risky to claim that the P-description given above accurately applies to any particular philosopher. Z's part, however, is a complicated and concrete one, and could not be played in less than two or three essays, since there are at least three discriminable senses of 'L-synonymy' which operate in the use of ordinary language. And to speak without specific citations of the problems of Professor P might seem too abstract and unconvincing — might seem to

rob his problems of their poignancy and to allow his arguments too little force. The problems involved in displaying the role of Z — problems of verisimilitude and space — I cannot overcome here. To overcome the other difficulty I am going to take a dangerous course: I am going to use some quotations from Professor Nelson Goodman's "On Likeness of Meaning".

Goodman's article has become something of a classic, and it is one fate of classics to be misused and misunderstood. With the danger of both faults in mind, I want to use Goodman in an attempt to understand the peculiar problems of a certain kind of P. The kind of philosopher to be considered is one who would ask, in his first sentence, "Under what circumstances do two names or predicates in an ordinary language have the same meaning?"[4] and who would then try to answer this question by specifying, "without reference to anything other than terms and the things to which terms apply, the circumstances under which two terms have the same meaning."[5] The peculiarities to be pointed out are: (A) That Goodman's strictly extensional answer could have nothing to do with Z's problems and decisions in the determination of L-synonymy, and (B) If Goodman's specification of the extensional circumstances under which two terms are alike in meaning is taken to define "LM-synonymy", and to furnish a criterion for its application, then it would not be possible for any 'we' ever to know whether "LM-synonymous" in this sense ever applied to any actual uses of L. No P could ever know that two L terms were 'synonymous' in Goodman's sense — unless he at one or two critical points abandoned or evaded the requirement that he refer only to terms and the objects which terms denote.

Since I do not wish to argue here that two terms in a natural language can be said to be exactly synonymous, or that statements in a natural language can be said to be absolutely necessary, it is not the particular negative point of Goodman's article that I must use to make the principal negative point of mine. I want to talk about a more general issue that Goodman raises in passing. Against the thesis that the meaning of a term is its extension, Goodman says it might be argued that "before we can investigate whether a given predicate 'P' applies to a given thing a we must know what 'P' means, and if the meaning of 'P' is its extension we must know the extension of 'P' — and therefore must know whether it applies to a — before we can set about finding out whether 'P' applies to a." But, Goodman counters, "this argument does not apply against the weaker thesis that two predicates have the same meaning if they have the same extension; for obviously we may decide by induction, conjecture,

or other means that two predicates have the same extension without knowing exactly all the things they apply to."[6] Yet it is possible to have some doubts about this counter. Is it so obvious that 'we may decide', if in making our decision we are permitted reference to nothing but terms and 'extensional objects'? Or do 'conjecture' and 'other means' cover a multitude of intensional indiscretions? One wonders, that is, about the viability even of the weaker thesis.

Consider Goodman's criterion of likeness of meaning: "If we [sic] call the extension of a predicate by itself its *primary* extension, the thesis is formulated as follows: two terms have the same meaning if and only if they have the same primary and secondary extensions."[7] 'Secondary extension' is adduced extensionally to distinguish the meaning of terms such as "unicorn" and "centaur" which in themselves do not differ in extension, denoting an empty class, and is formed by inventing single compound predicates such as "unicorn-picture" and "centaur-picture". But this does not affect the question to be asked of Goodman's criterion. Let us agree without argument that if two terms in a natural language ever have extensions thus related, they are synonymous, and that if their extensions are not thus related, they are not synonymous. We can allow, that is, that Goodman has in purely extensional terms given one sort of answer to the question "Under what circumstances do two names or predicates in a natural language have the same meaning?" The question that remains is this: How, in *equally extensional terms,* could we decide whether or not the required extensional circumstances did obtain? How could it be known whether any two terms did or did not have extensions thus related? Goodman may indeed have given us an impeccably extensional definition of 'synonymy', but one which also leaves us with no way of knowing whether or not the term thus defined ever applied to anyone's use of language.

We have already seen that a question about likeness of meaning in a natural language may ask for (A) an account of the principles of Z's practice in deciding whether two L terms are (L) synonymous, or for (B) a definition of "LM-synonymy". The latter question may be thought to require for its answer no more than a clear statement of the sort which Goodman's article gives — but a further question arises if one wishes to use LM in the analysis of L. If one hoped to use a meta-language to talk about ordinary language, then presumably one would have to know whether or when one's meta-linguistic terms applied to particular uses of the ordinary language. If *in principle* P could never know whether the extensional circumstances specified by Goodman's LM-definition of

"alike in meaning" were fulfilled by some actual uses of L, if for some reason he could never apply this extensional criterion of "synonymy" to a natural language, this would seem to constitute some limitation on the usefulness of the analytical language in question, however well-defined the relations among its LM terms.

A. That Z does not use this criterion — at least not with its extensional restrictions intact — is perhaps obvious; if he were to try, his problems in the determination of L-synonymy would turn out to have the same crux as P's problems in the application of "LM-synonymous." Goodman's definition of "synonymous", should Z attempt to use it, would presumably apply to certain pairings of L terms as names with objects purportedly named; the latter at least would probably be considered extensional objects. Goodman's definition of "synonymous" in P's use of it would presumably apply either to particular occurrences of shapes or noises taken to be L terms, or to the names of L terms, and to objects which the named terms — when out of quotes — purportedly named.[8] At least the first and last of these kinds of objects would probably be considered extensional objects.

If Goodman's criterion is to be used, then in the case of Z *and* in the case of P — whether Z is trying to decide if two terms of his language are L-synonymous, or P is trying to decide whether or not 'LM-synonymous' applies to some use of L — in *both* cases the use of Goodman's criterion requires that relations among purportedly named objects be assessed. Z would have to decide by reference only to 'Q' and 'R' and to the universe of extension, whether the primary and secondary extensions of these two terms were in fact alike, in order to apply to them the appropriate sense of 'L-synonymous'. P would have to decide by reference only to observed instances of the utterance or inscription of 'Q' and 'R' whether each of these utterances and compounds made of each in fact occurred only under extensional circumstances which were in fact like those under which the other occurred.[9] Thus whoever 'we' may be, to use this definition of 'synonymy' we shall have to make a kind of judgment of fact — an assessment of relations in extension. I want now to consider the question whether any 'we' could, *on strictly extensional grounds*, make the kind of judgment required.

B. To determine by definition the meaning of a term is in one sense to 'determine' the objects to which the term applies. This ideal or *a priori* definitional determination is not, however, equivalent to the discovery or the examination of a relation among matters of fact: this latter activity is 'determination' in quite a different sense. To determine, as a part of

one's definition of 'aardvark', that all aardvarks eat ants, is to decide that nothing will be properly named by 'aardvark' unless it can also be denoted by 'eater of ants' — one's determination of meaning is thus reflected in extension. But this determination is obviously a different process from the observation of some African burrowing mammal of the genus *Orycteropus* devouring any of certain social hymenopterous insects of the family *Formicidae*. To know the sort of thing that is named by a term if and only if the term correctly applies is not to have empirical information about the behavior of objects named, nor about their relations to other objects.

Suppose that we, as Z or P, decide that we shall try to use Goodman's criterion of likeness of meaning, and that we shall try to remain very austerely extensional in its use. We agree that "every difference in meaning will be reflected by a difference in primary and secondary extension",[10] but of course we can admit in evidence only reflections in extension and must disallow all suspiciously intensional consideration of what the reflections are reflections of. We must 'decide', by some extensional means, whether two predicates have the same extension: we shall be able to employ induction or conjecture only if they are respectably extensional induction and virginally non-intensional conjecture. (It would never do to spoil a virtuous end by vicious techniques.) We must make our decisions by reference to nothing other than terms, or to purportedly linguistic noises and shapes, and to the larger world of extensional objects which includes these.

Now how can we start — so to say — from this extensional end? How, in our extensional purity, are we to discover whether two terms have the same extension? How, in our innocence of intension — making calm little noises, staring blandly about, and virtuously repressing all thoughts of antecedent criteria of application, or better yet, chastely refusing to think at all — how are we to discover what relations obtain among objects which are named by a pair of terms only if they are named by the pair of terms? How, for that matter, if we are allowed to take into consideration only noises, lumps of ink, and the rest of the physical world, shall we be able to decide whether any object does or does not belong to the extension of any term? Goodman says that "'Centaur-picture' and 'unicorn-picture' *merely apply* to different objects just as 'chair' and 'desk' apply to different objects, and we need no more ask why in the one case than in the other."[11] But perhaps we need no *less* ask — if not 'why', then 'how'. And perhaps we shall find it difficult to avoid suspicion that the answer "merely" is less an answer than an attempt to

distract our attention while the closet door slams closed again on some unwanted, unacknowledged,, unnamed but indispensable skeleton of intensional meaning.

1. The limitations of a strict extensionalism will seem the more onerous when we recall that "the extension of a term consists of everything past, present and future to which the term applies."[12] Obviously any definition of any term, including Goodman's definition of '(LM) synonymy', casts its reflection in a certain way and once for all across the whole universe of extension, including all the uses of language, past, present and future. On the other hand it would not generally be thought that one could—short of omniscience or the abandonment of extensionalism — in practice survey what is thus eternally fixed. It would not generally be thought that the understanding of an expression (of L or of LM) required a veritable inspection, under the aspect of eternity, of the attribute of extension; even Spinoza did not hold that one's understanding could commence with this vision. It would not generally be thought possible for the tense-bound human knower actually to scan and to compare the several, various, presently hidden objects to which the term applies. One cannot — so to say — start from this end.

Z at least will be willing to acknowledge the exigencies imposed on judgment of fact by its character as an act in time, and confess the inelegant expedients to which it is forced by its temporal limitations. There may be, for instance, a sense in which the aspect of eternity can be taken in evidence, but that sense is not an extensional sense. Z does know that if a sentence is true, it is eternally true; he also knows that through all of past, present and future, the extension of a term is exactly whatever it is, and that its members have exactly the relations to other objects that they do. But his knowledge of which sentences *are* true, and of what objects in extension have which relations, will inevitably be in the form of judgments which take place at some particular point in time, and on whatever grounds are then available. Z has his methods for overcoming the lack of perfect coincidence between his knowledge and the realm of truth. If, for instance, he is asked on Monday morning — at which time the remote reaches of extension are not immediately or timelessly present to him — what it is to which 'Q' applies, and whether or not 'R' applies to each of the same things, he will find this question not impossible to answer. He will decide, and in this sense 'determine', the *sorts* of things to which the terms apply: he will, that is, fix certain more or less clearcut criteria for the application of 'Q' and 'R' and then, with these criteria

in mind, he may survey all time and all eternity in the only way he can, that is, imaginatively.

Now I do not claim that this kind of intensional determination and examination is in itself sufficient to certify the necessary truth of analytic statements. I think that it is not, and on this point — though for different reasons — I agree with Goodman. I do claim that some prior fixation of intensional meaning is indispensable to the useful and consistent application of terms. It is also true that criteria of application do furnish the grounds for one of Z's kinds of judgment of likeness of meaning: it is his most usual kind, and it is a useful kind, even if its use does not extend to the certification of necessary truth. [13]

It would be very agreeable to cite further agreement with Goodman on this last point; after all, he does say "In ordinary speech when we say that two terms have the same meaning, we usually indicate only that their kind and degree of likeness of meaning is sufficient for the purposes of the immediate discourse. This is quite harmless."[14] But even before we can express our relief, he goes on to say "But we must remember that the requirements vary greatly from discourse to discourse; often it is enough if two terms have the same primary extension; in other cases, identity in certain secondary extensions or others is also required. If we overlook *this variation* and seek a fixed criterion of sameness of meaning that will at once conform to these different usages and satisfy our theoretical demands, we are doomed to a perpetual confusion."[15]

Apparently, then, this variation in pertinence between primary and secondary extension is the only one that need be or can be taken into account: there is no retreat from extensionalism here. The P who would seek here for a fixedly extensional criterion of sameness of meaning that would at once conform to the different usages of ordinary speech and satisfy his theoretical demands would continue to deny all pertinence of Z's intensional recourse, would continue to insist that for Z to know the meaning of a term he must know its extension, and would not allow himself to understand or to analyze the uses of L unless he could do so exclusively in terms of the squiggles "Q" and "R" and the objects they (somehow) manage to denote. There is a kind of nobility in such self-denial, but, as is often true in such cases, it creates less natural and more general problems than it solves. The doom that would befall Z as a consequence of this denial is this: that disparity between what eternally is and his temporal knowledge of it — which sets the problem for empirical knowledge — would create, prior to the problem of knowledge, a perpetual problem in the determination of meaning which Z would not

be permitted to solve this side of omniscience. P's doom would be no less perpetual: if his extensionalism is consistent it will create for him an explanatory gap — between what P could allow himself to consider only as the occasional performance of peculiar noises and gestures by Z, and the entire physical universe — which could only be strenuously ignored, or filled with fortuitous coincidence.[16]

2. Goodman has told us that "obviously we may decide by induction, conjecture, or other means that two predicates have the same extension without knowing exactly all the things they apply to."[17] This might lead us to hope that if we did not have to worry about the *whole* of past, present and future, then we might be able to find some extensional sense of 'synonymy' that would conform to the use of ordinary language. Unfortunately this concession does not leave us much better off. Before we could by any means decide whether two terms did have the same extension, we should have by some means to decide at least a few acceptable members of both, or be prepared on some grounds to distinguish items that did from others which did not belong to the extension of each.

Suppose that we are asked whether 'Q' and 'R' are alike in meaning, and required for our answer to compare the objects to which 'Q' applies with the objects to which 'R' applies. Neither induction nor conjecture will be very helpful unless we can recognize suitable candidates for the application of 'Q' and 'R' when, along with all sorts of other objects, they appear. Induction is of use only *after* one has determined criteria sufficient to distinguish instances which are from instances which are not examples of the characteristic in question. But if recourse to such antecedent criteria is disallowed on the ground that their employment is not explicable purely in terms of 'Q' and 'R' (or «Q» and «R») and the extensional objects that 'Q' and 'R' denote, then induction will not produce any evidence as to which particular extensional objects are samples of what 'Q' and 'R' denote. Unless, that is, we are allowed to suppose that instances of Q always present themselves with the announcement "I am a Q!" — in which case we should still have to decide whether or not to take them at their word.

There is a difference between one's determination of the sort of thing to which he will apply a term — which is in effect a determination of criteria for its application — and his 'determination' of which particular things are things of the appropriate sort. This difference, and the prerogative status of the determination of criteria, cannot be eliminated by restricting the universe of denotation. Suppose, for example, that everything that was, is now, or ever shall be either a basilisk-picture or a

cockatrice-picture is now in a pile of pictures on your desk.[18] There is no doubt that some objects on your desk make up the entire (primary) extension of 'cockatrice-picture' and that no object not on your desk is any part of the (primary) extension of 'cockatrice-picture'. The same is true of 'basilisk-picture'. The question is put: *(a)* When are we entitled to say that 'cockatrice-picture' is synonymous with 'basilisk-picture'? The answer can be given: When every individual instance of the denotation of 'cockatrice-picture' is likewise an instance of the denotation of 'basilisk-picture', and vice versa. The two terms can be called 'synonymous', thas is, when their extensions are the same. The further question may arise: *(b)* Is 'cockatrice-picture' synonymous with 'basilisk-picture'? That is, are all the cockatrice-pictures on the desk basilisk-pictures? Is each thing to which 'cockatrice-picture' applies also a thing to which 'basilisk-picture' applies? To answer this it would seem necessary to decide *(c)* Which things on this desk are cockatrice-pictures, which are basilisk-pictures, which griffin-pictures, which wife-and-children-pictures, etc. We should have to decide which objects on the desk belonged to the extension of 'cockatrice-picture', which of 'basilisk-picture' which of 'martlet-picture' and so on, but this could only be accomplished if *(d)* We had determined some *criterion* for the sort of thing to which 'basilisk-picture' applied.

That definition of 'synonymy' which answers question *a* is a necessary condition but not a sufficient condition for discovering the answer to question *b*. That determination of criteria, that definition of *(L)* terms which would meet the conditions set by *d* is likewise a necessary but not a sufficient condition for answering question *c*, and it is at least a necessary condition for answering *b*. (There are senses of '*(L)* synonymy' — weak, vulgar, and useful senses — in which *d* is a necessary and a sufficient condition for the determination of *b*.) The fact that any of these — *a*, *b*, *c*, or *d* — may be offered in answer to questions about the determination of synonymy in a natural language should not lead us to overlook the fact that the sense of 'determination' is different in each case.

III. That these obvious points have been overlooked by a philosopher so keen as Goodman will not readily be believed. It is far more likely that he regards them as unimportant. Notwithstanding the contrary suggestions of the first pages of "On Likeness of Meaning", it can only be supposed that he is not concerned with likeness of meaning in any sense to which description of usage or the techniques of application have any pertinence; he does say "*Under what circumstances* do two names . . .", and it is true that everything happens under circumstances which extend far beyond our knowledge. Goodman may be interested

only in the 'circumstances under which' language is used, and not concerned with the use of language; perhaps he is interested only in what the infinite universe around us is like when we happen to be talking, and not in the reasons why we say what we do. This restriction of interest would account for statements to the effect that terms 'merely apply', and would avoid any counterfactual assumption that extensional restrictions were observed in the use of ordinary language.

P could, that is, construe Goodman's statement of the circumstances under which two terms are alike in meaning as a purely semantic statement to the following general effect: whenever likeness of meaning occurs in a natural language, such and such extensional circumstances likewise occur. By saying nothing more than this, P could avoid the impossble requirement that he 'determine', on strictly extensional grounds, when there existed in fact a parallelism between occurrences of pairs of terms and particular configurations of extensional circumstance. This difficulty would simply fail to arise — unless P tried to use his extensional meta-language to talk about ordinary language. Then, of course, he would find that he could never say when or whether any two terms ever were in this sense 'alike in meaning'.

But suppose that Professor P is willing to refrain from such use, that he is content to say only something of the following sort: In all cases in which it could truly be said (never mind on what grounds or by whom) that two terms are alike in meaning, it is also true (never mind whether anyone could judge it so) that their extensions are alike, and vice versa. Such a statement has nothing to do with actual attribution of synonymy, and it succeeds in mentioning only terms and those objects to which the terms refer (never mind how). The next question is: What kind of statement is this?

One can see two possible interpretations of such a purely extensional statement of the purely extensional circumstances under which two terms are alike in meaning. A. It might be understood as a *(LM)* definition which stipulates the syntactic relations of "term", "extension", and "alike in meaning" within some particular meta-language. In this case, no exception need be taken — unless we inquire into the further and external question of its use, and discover again that it has none.

B. If P were unwilling to consider Goodman's statement of the circumstances under which two terms are alike in meaning as part of a purely decorative meta-language, perhaps he would construe it as a metaphysical or, as we should say now, an ontological statement. Perhaps Goodman means to inform us of the relation of language to reality, or

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something of this sort. This would be a very interesting sort of information to have, and Goodman himself sounds nostalgic when he says, in reference to the question of likeness of meaning in a natural language, that "one of the earliest answers is to the effect that two predicates have the same meaning if they stand for the same real Essence or Platonic Idea; but this does not seem to help very much unless we know, as I am afraid we do not, how to find out whether two terms stand for the same Platonic Idea."[19]

That metaphysics makes strange bedfellows is a story old as Socrates: we have already seen that if one remains rigidly extensional, then one can no more know the Goodmanic Extensions of terms than one can know their Platonic Ideas. One has been led to suppose that to be even a little intensional is like being a little pregnant, and that one's extensionalism must be pure to be worth loving — but to a pure extensionalism the Real Extension of terms must remain just as little knowable as their Real Essences.

So regrets would seem again in order: whoever 'we' may be, we shall have to say, in reference to the question of likeness of meaning in a natural language, that one of the latest answers is to the effect that two predicates have the same meaning if they have the same primary and secondary extension; but this does not seem to help very much unless we know, as I am afraid we do not, how to find out whether two terms have the same primary and secondary extension.

NOTES

[1] White, M. G., "The analytic and the synthetic: an untenable dualism", in *John Dewey: Philosopher of Science and Freedom*, (Hook, S., editor), N.Y.: Dial, 1950, 316–330; quotation from page 321.

[2] White, *ibid*, 329.

[3] Cf. Goodman, Nelson, "On likeness of meaning", reprinted in *Semantics and the Philosophy of Language*, (Linsky, L., editor), Urbana, Ill.: University of Illinois Press, 1952, 67–74: " . . . . we have answered our question without reference to anything other than terms and the things to which they apply . . . ." (page 71); Cf. Quine, W. V., *From a Logical Point of View*, Cambridge, Mass.: Harvard University Press, 1953: "But can we condone a language which contains such an adverb [as 'necessarily']? Does the adverb really make sense? To suppose it does is to suppose that we have already made satisfactory sense of 'analytic'. Then what are we so hard at work on right now?" (page 30)

Goodman (*ibid*, 73) does allow that "requirements vary greatly from discourse to discourse", and White (*ibid*, 325) does suggest the question "Who are 'we'?"

[4] Goodman, *ibid*, 67.

[5] Goodman, *ibid,* 71.

[6] Goodman, *ibid,* 69.

[7] Goodman, *ibid,* 71.

[8] For the moment we can use French quotes to set off purportedly linguistic shapes or noises, single quotes to distinguish terms, and double quotes to mark the names of terms.

[9] Cf. Goodman, N. and Quine, W. V., "Steps toward a constructive nominalism", *Journal of Symbolic Logic,* 12 (1947), 105–122. If P wanted to use the *names* of *L* terms, he would have not only (somehow) to identify some sets of extensional circumstance as the same (whatever that would mean) or like those under which the named term had previously been applied, or of which it had previously been held true. This would be hard enough, but before he could do this P would also have by some (presumably strictly extensional) means to ascertain whether certain concrete objects – the noises or lumps of ink «Q» and «R» – were in fact instances of the terms named by "Q" and "R".

[10] Goodman, *ibid,* 72.

[11] Goodman, *ibid,* 71, italics added.

[12] Goodman, *ibid,* 69.

[13] The certification of necessary truth can be accomplished, but it requires more elaborate means, in particular a reference to one or another of two kinds of explicit linguistic rule.

[14] Goodman, *ibid,* 73.

[15] Goodman, *ibid,* 73, italics added.

[16] "It is a frequent fallacy of those who are inclined to tear every issue to rags that they would confine knowledge within some kind of phenomenalistic limitations but allow themselves to speak like gods, who, by standing outside these limitations, can look down upon us poor humans struggling to know and observe the futility of our endeavors. Characteristically they thus involve themselves in the implicit contradiction that if their critical observations were valid, then what they assert would be a thought which they could never substantiate or even think." Lewis, C. I., *An Analysis of Knowledge and Valuation,* LaSalle, Ill.: Open Court, 1946, 330–331.

[17] Goodman, *ibid,* 69.

[18] It does not matter that these are the sorts of things Goodman uses as the secondary extensions of terms like 'basilisk' and 'cockatrice'. I am talking about the primary extensions of 'basilisk-picture' and 'cockatrice-picture' – I need not consider their secondary extensions – because I want to narrow the universe of extension for this example, and it is plausible to say that the top of a desk could hold the entire primary extension of these terms.

[19] Goodman, *ibid,* 67.

EXPECTATIONS AND INTERACTION PROCESSES

by

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In sociology the word *interaction* has a very prominent place.* Its use is marked by names like Simmel, von Wiese, Park, and Mead (the Chicago School) and it plays a very conspicuous role in the Parsonian approach to sociology, as "interaction" is the keystone in the concept of "social system" (14, p. 7). However, one cannot possibly say that a theory of interaction exists at the present stage. "Interaction" is a minimal concept; very little is said so that it may serve as a fundamental concept. Very much is known about interaction in a more specific sense, e.g., interaction in small groups, in different institutions and organizations, in specific role-relations, etc., but little is said about just "interaction". It seems that this is at least partly due to insufficient conceptualization, because only through conceptualization can the latent richness of a concept be made manifest, and only through this process can possible theorems involving the concept be at least conceptually justified. To assist in this process, a symbolic system will be developed to facilitate the expression of thoughts that are hard to express in a surveyable way in ordinary language.

We shall assume here that interaction may justly be regarded as a binary relation between two action systems. These two systems can as usual be conceived of as two individuals, two sets of status-holders, two human groups, or for instance one individual and one group. We choose one of the systems as the point of reference and call it Ego, the other one we call Alter — but the relations between the two systems

will be analyzable in a completely symmetrical manner. Further a "disinterested" observer may be brought into the picture, but it is usually considered unsatisfactory to describe and try to explain an interaction process from the observer's point of view alone. The most fruitful point of departure for a description and a theory of an interaction process seems to be a combination of the two subjective views, Ego's and Alter's, and the "objective" view of the observer. The subjective analysis tells us how the actors *perceive* what is going on; the objective analysis what actually *is* going on, particularly in the actors' perceptions.

The symbols we shall introduce will be symbols for what *is,* so far as competent observers can ascertain it. There will for instance be a symbol for Ego's perception of Alter, $E(A)$. By this is meant the observer's description of how Alter is perceived by Ego, the Alter *für mich* relative to Ego, not the Alter *an sich* (relative to the observer) which we shall symbolize by A. Further, all symbols introduced are symbols of *sets*. For instance, Ego's prescriptions to Alter will be symbolized by $E \rightarrow A$; by that is meant the set of all prescriptions held by Ego about Alter. We could have chosen other logical types, but this has certain advantages as will be made clear later on.

To introduce the symbols systematically, some reflections on the concept of 'interaction' must be given. In a sense, the most important element in any analysis of the concept is *time*. In interaction, the *perceptions* of the present are mingled with the more or less conscious *memories* of the past and the more or less explicit *expectations* of the future. As we shall be more than content to symbolize what goes on at a given point in time, we shall concentrate on perceptions and expectations and regard memories as traces of past experiences built into both. "Perceptions" will be regarded as relatively unproblematic, but "expectations" require further analysis.

Expectations will be conceived of as *standards of evaluation,* located in the mind of one individual and used to evaluate attributes and actions of oneself and other individuals; the first individual being the *subject* of the expectation, the second the *object* of the expectation. The concept has a clear time connotation insofar as it does not refer to events in the past (except in the sense of postdiction). The expectation must exist prior to the relevant phenomena, but it is the coincidence in time of the expectation with the phenomenon which makes for the possibility of a comparison of the object with the standard implied by the expectation. Comparison of an expectation with the relevant object can in principle yield three different kinds of results (the first two varying in degree):

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1. The comparison may give a conclusion of *consonance;* the object is in agreement with the standard.

2. The comparison may give a conclusion of *dissonance;* the object is in disagreement with the standard.

3. The comparison may give a conclusion of neither consonance, nor dissonance; the object may be said to be *neutral,* or the standard is *meaningless* or *irrelevant* as applied to the object.

All expectations have these types of conclusion in common, but they vary considerably in terms of *what are the consequences of dissonance.* The dissonance means that somehow there is a disagreement between the object evaluated and the expectation. If we take it as axiomatic that this kind of dissonance is intolerable and will lead to adjustment mechanisms, it is obvious that there are two extreme kinds of such mechanisms:

1. Efforts are made to *change or redefine the object* so that it agrees with the expectation.

2. Efforts are made to *change or redefine the expectation* so that it agrees with the object.

Before we link this in with interaction processes, the distinction will gain in meaning by being applied to a passage in W. O. Quine's article "Two Dogmas of Empiricism" (15, pp. 42 ff.). After his analysis of 'analyticity', he introduces the metaphor where "total science" is regarded "like a field of force whose boundary conditions are experience" — "a man-made fabric which impinges on experience only along the edges". Then he writes (15, p. 43):

> If this view is right, it is misleading to speak of the empirical content of an individual statement — especially if it is a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic state-
> 1. ments, which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the phase of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called
> 2. logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the

excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle?

The segments indicated in the passage quoted correspond to the two possible "adjustment mechanisms" in what we have called cases of dissonance between object and expectation, described in Quine's jargon as "a conflict with experience at the periphery". Quine introduces the idea of a continuum between the two extremes, a continuum which can be regarded as a mapping of degrees of analyticity, and his basic idea seems to be that a "statement" is not analytic or synthetic *per se,* but its degree of analyticity is dependent on "considerations of equilibrium affecting the field as a whole".

In these considerations there is ample opportunity for less explicit or less rational motives to enter. In the sociology of knowledge a focus would perhaps be on what Galbraith so aptly calls "the conventional wisdom" and describes as (5, p. 9) "a prime manifestation of vested interest. For a vested interest in understanding is more preciously guarded than any other treasure. It is why men react, not infrequently with something akin to religious passion, to the defense of what they have so laboriously learned." This, of course, is a factor which will motivate for the use of method no. 1 in cases of dissonance.

In the analysis of interaction processes we are not concerned with "the totality of our so-called knowledge or beliefs" (15, p. 42), but rather with the totality of desires, hopes, beliefs, norms etc. resting in either of the participating actors, and geared towards the other. But with this change in field of interest not much changes in the analysis. We regard dissonance as a condition which by definition motivates the actor to some kind of consonance-seeking activity, or in other words: to a reequilibration of the relation between his expectations and his perceptions. Since Fritz Heider published his article on "Attitudes and Cognitive Organization" in 1946 (7) other authors have made important contributions to our understanding of these mechanisms of equilibration; like Osgood and Tannenbaum (12), Cartwright and Harary (2), Leon Festinger (4) and Rosenberg and Abelson (17).

Expectations of the first kind, where dissonance is avoided by changes or redefinitions of the object, include as the most important special case for our purpose the case of the *normative expectation* or *prescription.* Cases of dissonance are called cases of deviance and Alter is somehow defined as *wrong;* avoidance of dissonance is achieved by means of the

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vast variety of disparate mechanisms lumped together by sociologists in the category "social control". Another way of avoiding dissonance in the patterned ignoring of the deviant act, as in the case of the teacher who pretends not to notice his pupil's defiance of one of his prescriptions, or rather, tries not to let the pupil or some third person know that *he* knows.

Expectations of the second kind, where dissonance is avoided by changes or redefinition of the expectation include as the most important special case for our purpose the case of the *cognitive expectation* or *prediction* (where we, as mentioned, include postdiction). Cases of dissonance may be called cases of disconfirmation or falsification of the expectation, which is somehow defined as *false;* avoidance of dissonance may then be achieved by a suitable revision of the expectation, for instance in the way prescribed by the usual canons of scientific activity or some simplified common-sense version of these rules.

In a concrete case of interaction all kinds of intermediate outcomes of cases of dissonance are conceivable. When the dissonance is perceived, a mild control may ensue, but at the same time a partial revision of the expectation may take place. This may be given the rationalization "the prescription was unrealistic, one cannot expect (in the normative sense) *that much* of a human being." Expectations in the field of sexual behavior may, for instance, have acquired that character for many persons in many situations in the post-Kinsey period, i.e., a gradual sliding in meaning for the holder of the expectation from pure prescription to pure (and very modifiable) prediction of sexual behavior.

How then, can we conceive of this continuum from the pure norm to the pure belief? This is the same problem in its abstract form as the one Ofstad discusses (11, p. 49) as possible relations between Right and Wrong. We feel inclined to choose his alternative 2, with pure cases at either end, and mixtures with various degrees of normativity in between. It remains to be said that the degree of normativity is just as little inherent in the expectation itself as the degree of analyticity, but is rather a function of the structure of attitudes and beliefs held by the individual and how the individual perceives possibilities of avoiding dissonance. Corresponding to the classical tautology "which holds come what may" would be the most deeply held norms, like norms regarding life and death or honour, either of oneself or of a salient collective. Like Quine, we see no immediate way of translating these ideas into operational devices that could give us, for a given

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person, a given situation and a given expectation, the degree of normativity. However, there should be great possibilities for research along the line suggested by Rosenberg and Abelson when they construct mathematical models for the calculation of the minimal change that will restore equilibrium in a system of beliefs and norms temporarily in dissonance.

There is one additional reason why we feel this way of bridging the classical gap between prescription and prediction to be important. If we introduce time, and change to a more dynamic analysis, awareness is focussed on the classical problem in sociological theory of the "emergence of norms". Common-sense illustrations are plentiful. Take as a case an Ego who observes an Alter throwing a ball with his left hand several times, and hence infers that "Alter is left-handed". After some time, this becomes a cognitive expectation, and when he next time observes Alter cutting bread with his left hand he gets a feeling of consonance: "my expectation was true". But, when he after some more time observes Alter signing checks with his right hand (or, for that matter, throwing a ball), he may exclaim with an undertone of reproach: "I thought you were left-handed!" Cognitive expectations direct our relations to the external world; if we are used to go by them, cases of dissonance are disturbing and may lead to a quick, more or less explicit calculation: where shall I make the change? If he "chooses" to change the object, and the object is social, what else do we have than a case of social control? And in the example given a prescription seems to have emerged with the function of "freezing" what has become a cognitive habit so as to minimize the chance of present or future adjustment.

As a final reflection on the fruitfulness of bridging the gap between predictions and prescriptions the position taken by Arne Næss (9, p. 53) should be mentioned, *viz.*, "both rejecting the decisive force of *a priori* and of *a posteriori* argumentation in answering the question of truth-value of basic norms." From Næss' agnostic position (*ibid.*, p. 44): "I am neither certain that there are normative truths to be verified or confirmed, nor sure that there are not" an avenue (or rather, a narrow track) leads perhaps to the application of the adjective "normative" to some subclass of even the expectations where consonance is interpreted as *confirmation*. We shall not explore these possibilities any further, but indicate the possibility of dropping the adjectives "normative" and "cognitive" from our vocabulary, and concentrate on the analysis of dissonance-avoiding activities.

We now introduce a minimum of symbols, and start with *dramatis personae:*

<div align="center">E (ego)   A (alter)</div>

and then give symbols for perceptions and expectations where we shall follow Parsons' lead and introduce the supercategory *orientations:*

E(A) Ego's *perception* of Alter
      Ego's *description* of Alter
      Ego's *image* of Alter

and E → A Ego's *normative expectations* to Alter
      Ego's *prescriptions* to Alter
      Ego's *norms* to Alter

and E[A] Ego's *cognitive expectations* of Alter
      Ego's *predictions* of Alter
      Ego's *beliefs* about Alter

where we take the three definitions of the symbols to be synonymous. We shall, however, prefer to use the word "expectation" with an adequate adjective often, because of its connotation of future in addition to present and to keep in mind the continuum between norms and beliefs. It should be pointed out that the word "perception" to us has no connotation of standard, but rather that the percept (i.e., E(A)) is the raw material which is fed into the expectation for comparison and evaluation. This, of course, facilitates the adjustment of the external world to a deeply held expectation, as it is considerably more easy to adjust the Alter *für mich* than the Alter *an sich*.

In a dyad, with two possible subjects and two possible objects of an expectation and two different kinds of expectations, we get $2^3 = 8$ as the total number of *first-order expectations*. These, together with the four relevant perceptions are given in the following table:

	EGO (Object)		ALTER (Object)	
EGO (Subject)	E(E) Self-Description	E[E] Self-Prediction	E(A) Other-Description	E[A] Other-Prediction
	E→E Self-Prescription		E→A Other-Prescription	
ALTER (Subject)	A(E) Other-Description	A[E] Other-Prediction	A(A) Self-Description	A[A] Self-Prediction
	A→E Other-Prescription		A→A Self-Prescription	

It seems reasonable to pursue the thought a little further. By *second-order expectations* we shall mean expectations about expectations, for instance the very important expectation (cognitive) of a physician to the patient that the latter will expect (normatively) the former to be competent. Correspondingly, *third-order expectations* are easily seen to be not only empty baggage. As will be shown, second-order expectations are indispensable in any formal analysis of the concept of interaction, and as an example of a third-order expectation can be mentioned a phrase from a Norwegian novel: "The important thing was not that he knew she was in Paris, but that he didn't know she knew that he knew it."

As an example of a more subtle kind of interaction where expectations beyond the second order are needed for a fruitful analysis can be taken the following situation. Imagine a teacher who enters a new class for the first time; she is the Ego of the analysis and the many-headed class is the Alter. She has her image of the class and her norms to the class, $E(A)$ and $E \rightarrow A$. But she is new to the class, she does not know what kind of game the old teacher played with the children, and hence she has difficulties in perceiving what they expect of her, $E(A \rightarrow E)$. Among these difficulties there is a sub-category which is particularly important, *viz.*, $E(A \rightarrow (E \rightarrow A))$, her perception of what they expect her to expect from them, normatively speaking. Probably, $A \rightarrow (E \rightarrow A)$ is to a large extent determined by $E^* \rightarrow A$, where E^* is the old teacher — hence, a new teacher always tries to find out what the old teacher prescribed from the belief that this is to a large extent what the class will prescribe her to prescribe if there are reasons to believe that the old teacher had sufficient legitimate power to define the status-relations in the class.

The number of second order orientations is quite high: $2 \cdot 3 \cdot 2 \cdot 3 \cdot 2 = (2 \cdot 3)^2 \cdot 2 = 72$. Similarly, any n'th order orientation will involve n orientation symbols (with three possibilities) and $n + 1$ actor symbols with 2 possibilities, so that we get $3^n \cdot 2^{n+1} = 2 \cdot 6^n$ as the total number of orientations. However, these symbolic generalizations will very rapidly transcend what the human mind can imagine; even fifth order expectations are difficult to grasp. But even though they may have no correlate in the minds of the actors, these constructs may still prove useful in analysis.

Three basic questions about an orientation can be read immediately from the formulas:

1. *Who is the subject of the orientation?* Given by the first individual-symbol.

2. *Who is the object of the orientation?* Given by the second individual-symbol.

3. *What kind of expectation?* Given by the first orientation-symbol.

Thus, $A(E \rightarrow A(A))$ is a description held by Alter about Ego, and more particularly about what image he prescribes that A shall have of himself, Alter. This particular expectation seems to be of considerable importance in studies of all kinds of human-centered professions, e.g., the social worker. Correspondingly, $A(E \rightarrow A(E))$ seems to be quite important in love-relations, or for that matter in most human relations. In these formulas, E and A are of course interchangeable as long as it is done consistently. Or, let us take an important factor often causing much trouble in the relations between a lawyer and his client. It is (perhaps) a part of the professional ethics of lawyers that they shall do their utmost to "win" the process. In order to win, a *belief* in the possibility of victory may to some extent be a psychological prerequisite. Whether or not the client expects the lawyer to hold this prediction, the lawyer may have that image of the client and take on a certain optimistic air to conform. With Ego as the lawyer, we are thus dealing with $E(A \rightarrow E[E])$.

As a final example, let us look at this quotation from Goffman (6, p. 2) about how an individual presents himself: "He may wish them to think highly of him, or to think that he thinks highly of them or to perceive how in fact he feels towards them, or to obtain no clear-cut impression". The first three have to do with gradually more complex norms, *viz.*, $E \rightarrow A(E)$, $E \rightarrow A(E(A))$ and $E \rightarrow A(E(E(A)))$, where we interpret "in fact" to mean how "he" (Ego) himself perceives that he feels towards "them" (Alter). Obviously, the first of these three symbols defines a broad class of norms regarding how "they" shall perceive "him", where the present case ("to think highly") is a subclass. But when we come to the last part of Goffman's phrase something more definite can be said: $E \rightarrow (A(E) = \emptyset)$, i.e., that their image shall be empty. Many other instructive examples can be found in texts by Heider (8) and Sullivan (20).

We shall not pursue the scholastic ideal of presenting all combinatoric possibilities, but only point out that they are easily found and may be useful for the theorist as a systematic way of locating analytic problems.

If our symbols are to be of any merit at all, they must at least help us to locate some traditional sociological problems and, as mentioned in the beginning of the section, help us to understand more fully the nature of the interaction process. We shall then conceive of interaction as a process where *comparisons are constantly being made,* between expectations and the objects of the expectations, so as to adjust the objects or adjust the expectations until consonance is reached. These comparisons can be made by the outside observer; in that case comparisons are made between, for instance, his description of $E \rightarrow A$ and A. Much more interesting than this objective approach, however, is the subjective approach where the focus is on intra-actor comparisons, as these comparisons form the raw material the interaction process feeds on. In this case, perceptions held by the actor are compared, e.g., $E(E \rightarrow A)$ with $E(A)$.
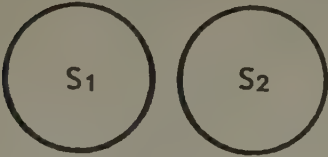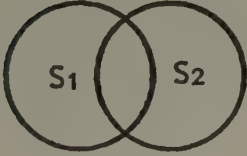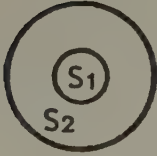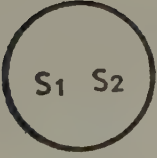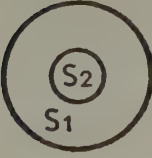
At this point a small trick will be introduced, as it will greatly facilitate the use of symbols. To carry out the last comparison mentioned a set of descriptions of norms and a set of descriptions of actions or other attributes will be compared. This is unfortunate, as the two sets are not subsets of the same total set, which is the condition for the full application of the class calculus. If, however, we interpret $E(E \rightarrow A)$ to mean the *description of all actions prescribed to Alter by Ego;* in other words, what Ego conceives of as being the "right" actions for Alter to perform, this set can immediately be compared with his perception of what Alter actually has performed. All our symbols, regardless of what order expectation they represent, denote (perceived) *sets* of acts or attributes, because of the interpretation of a prescription as a set of prescribed acts.

Obviously, such sets are constantly being compared in the interaction process for possible overlap, inclusion, equality, etc. To start out with an important comparison as an example, let us take the two sets

S_1: $E(E \rightarrow E)$ The set of actions Ego perceives that he prescribes for himself

S_2: $E(A \rightarrow E)$ The set of actions Ego perceives that Alter prescribes for him

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We have the following five relations:

Visual Diagram	Symbolic Presentation	Interpretation
1.	$s_1 \cap s_2 = \emptyset$	To Ego it seems that they prescribe entirely different actions, hence that there is no immediate basis for interaction.
2.	$s_1 \cap s_2 \neq \emptyset$ $s_1 \cap \overline{s_2} \neq \emptyset$ $s_2 \cap \overline{s_1} \neq \emptyset$	Here there is an overlap and a basis for smooth interaction from Ego's point of view if the partners are willing to stick to the set $s_1 \cap s_2$.
3.	$s_1 \subset s_2$	Ego perceives that the actions he prescribes to himself are within what Alter prescribes; hence that he can stick to his own norms.
4.	$s_1 = s_2$	The ideal situation of perfect agreement, but no basis for change caused by conflict.
5.	$s_2 \subset s_1$	The opposite of the third: Ego perceives Alter's prescriptions as valid because they are part of his own.

If we compare situations 3 and 5, it seems that they give us an explication of the terms "other-directed" and "inner-directed" introduced by Riesman (16, p. 40 f.). Both of Riesman's types have sets of prescribed actions, and both types undoubtedly distinguish between what they perceive that they themselves prescribe and what they perceive that

others prescribe for them. The difference can perhaps be expressed as follows:

For the *other-directed* $E(E \rightarrow E)$ has to be located within $E(A \rightarrow E)$ because if it is not, there is no guarantee of validity, as the source of legitimation of a norm is Alter. The limiting case is $E(E \rightarrow E) = \emptyset$.

For the *inner-directed* $E(A \rightarrow E)$ has to be located within $E(E \rightarrow E)$ because if it is not, there is no guarantee of validity, as the source of legitimation of a norm is Ego. The limiting case is $E(A \rightarrow E) = \emptyset$.

Of course, in both cases one set may not be entirely included in the other but the essence of the explication is that over time Ego's perception will be such as to include the one in the other. The inner-directed person's feeling of security is linked to his perception of prescriptions as coming from himself *or coinciding with* prescriptions coming from himself, and correspondingly for the other-directed person. Another thing is that the inner-directed person in some period of his life must have been other-directed, as we do not believe that norms originate in anything but human interaction.

The example shows how these sets can be compared and the kinds of analyses in sociological theory they may initiate. Essentially, the comparison is a comparison of the *extensive* properties of the sets only: we are only asking what they have in common, which set is included in the other, not what is the nature of the elements in the overlap as distinguished from the other elements, etc. This is a tremendous weakness but necessary because of the extensional nature of the set-calculus.

However, we need some kind of measure of the discrepancy between the two sets being compared. Ideally, the measure should be at its maximum in case 1 and have the value 0 in case 4, and it should deal with the two sets in a symmetric way (but asymmetric measures may also be valuable). It seems that the *symmetric* difference, $S_1 \quad S_2$, is the logical candidate as a measure; i.e., the sets of points that are *either* in S_1 *or* in S_2, but not in both, nor in neither.* We have

$$M(d) = S_1 \triangle S_2 = S_1 \cup S_2 - (S_1 \cap S_2) \text{ (Measure of Discrepancy)}$$

which gives us

Case 1: $\quad M = S_1 \cup S_2, \; S_1 \cap S_2 = \emptyset$

Case 2: $\quad M \subset S_1 \cup S_2, \; S_1 \cap S_2 \neq \emptyset$

Case 3: $\quad M = S_2 - S_1$

* The same idea has been suggested by Mr. Per Bjørnstad for preference relations.

Case 4: $M = \emptyset$

Case 5: $M = S_1 - S_2$

If we had some kind of numerical measure function, $N(S)$, we could norm with $N(S_1 \cup S_2)$ and get a measure varying between 1 and $N(S \triangle S) = N(\emptyset) = 0$. This measure would, however, not distinguish between cases 2, 3 and 5.

With this apparatus developed, we can turn fully to the task of explicating "interaction", using the two fundamental texts by Parsons as source material, The Social System (13) and Towards a General Theory of Action (14). We find the clearest exposition of what "interaction" might possibly mean in the latter (14, pp. 153 ff.):

1. The social object is not, however, an inert source of gratification, but reacts toward him, so that there enters a conditional element into the fullfillment of expectations. Alter has expectations of Ego and vice versa; this is what we have already called

2. a "complementarity of expectations." At the very beginning the infant is perhaps almost an environmental object to the adult. But this aspect changes quickly, a reciprocity of responsiveness builds up, the infant's smile calls forth responses, and organization along the axis of gratification and renunciation becomes more differentiated. As all this happens, he begins to play a role in the social system; that is, he acts in accordance with expectations, just as the adult does.

3. The essential element in the role is the complementarity of expectations. The outcome of ego's action, in terms of its significance to him, is contingent on alter's reaction to what he does. This reaction in turn is not random but is organized relative to alter's expectation concerning what is "proper" behavior on ego's part. The reaction, then, is organized about the problem of whether, and to what degree, ego "conforms"

4. with alter's expectations of what he should do. At the very beginning the expectations may be purely predictive, but very soon they acquire a normative content. (This normative aspect has indeed been included in the concept of expectation from the start.)

We have cut the statement into four segments and shall try to find out what these segments mean translated to the symbols introduced.

1. This statement seems to be somewhat misleadingly formulated. It is hardly at this level that the difference between the non-social and the social object of orientation becomes clear. Undoubtedly, there is a difference between shaking a cocktail and shaking the host's hand, but there is also a similarity: in both cases Ego (who shakes) will try to take into account the fact that the object, glass or hand, will react towards the shaking. His gratification is conditional on this reaction, e.g., he must have some idea about the maximum pressure the glass or the hand can withstand. At this level, then, only $E[A]$ and $A[E]$ are brought in, and we see no need to waste the valuable word "complementarity of expectations" on such a trivial phenomenon.

2. By now, more seems to happen. Though it is not clear whether cognitive or normative expectations are implied, we assume that the expectations alluded to are normative ones (see statement in parentheses, end of segment 4). If Ego is the child, the statement that "he begins to play a role in the social system" apparently means two things: that the adult Alter has prescriptions to him ($A(A \rightarrow E)$ $\neq \emptyset$) and that the child actually at least to some extent acts in accordance with them

$$A(A \rightarrow E) \,\triangle\, A(E) \neq \emptyset$$

although it is unclear whether this comparison actually takes place in the mind of Alter or in the child or perhaps in the observer only.

3. At this point, a new effort is made to clarify the concept of "complementarity". We have cut out four sentences, and it seems that the last two sentences carry the burden of the definition. "Alter's expectation concerning what is 'proper' behavior on ego's part" clearly is given by $A(A \rightarrow E)$, and what is being said, essentially, is that Alter's reaction is dependent on his comparison of the two action-sets $A(A \rightarrow E)$ and $A(E)$. The first two sentences seem to be restatements of what has already been said many times, that Ego's orientation is dependent on $E(A)$, over time.

There remains the problem of whether this should justly be called "complementarity". Even at this point we would prefer not to waste this useful term on what seems just to be implicit in the meaning of a normative expectation, for of what use should a normative expectation be if it were not for comparisons with the object of the expectation?

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4. This paragraph seems to be of the utmost importance, especially as it contains a theorem that should be empirically testable. What is said is that expectations may start out as purely cognitive, but over time gradually take on a normative character. If we introduce the symbol $\mapsto$ to mean "implication over time" (if the antecedent is true at some time t_n then the consequent will be true at some time t_{+pn}), we can get hypotheses of the following kinds:

$$E[A] \neq \emptyset \mapsto E \rightarrow A \neq \emptyset$$

where for A we can substitute E and vice versa (four hypotheses altogether). What is being said is that *a prediction of a social object will turn into a normative expectation and that the holder of the prediction will become aware of these expectations.* Essentially, this is the theory for *the emergence of norms,* which we have already argued.

To get some more insight, let us quote another passage from the same book, this time from the introductory part (14, p. 15):

1. Ego does not expect the behavior of a nonsocial object to be influenced by expectations regarding his own behavior, although, of course, ego's behavior is influenced by his expectations concerning the behavior of the nonsocial object. It is the fact that expectations operate on both sides of the relation between a given actor and the object of his orientation which distinguishes social interaction from orientation to nonsocial objects.

2. This fundamental phenomenon may be called the complementarity of expectations, not in the sense that the expectations of the two actors with regard to each other's action are identical, but in the sense that the action of each is oriented to the expectations of the other. Hence the system of interaction may be analyzed in terms of the extent of conformity of ego's action with alter's expectations, and vice versa.

1. This passage corresponds to no. 1 above, but seems to be a more happy formulation. When Ego *acts,* he only takes into account his predictions about Alter, $E[A]$ (in addition to the trivial $E(A)$). But when he *interacts,* he also takes into account his perception of Alter's expectations to him; that means $E(A[E])$ and $E(A \rightarrow E)$.

When we take into consideration that Parsons indicates the possibility that the "expectations may be purely predictive" (statement no. 4 above), it is unclear whether all the expectations alluded to in the passage are cognitive or normative. So why not say that they can be both? — and distinguish between

cognitive interaction   based on E(A), E(E[A] and E(A[E]) and correspondingly for Alter and

normative interaction   based on E(A), E(E → A) and E(A → E) and correspondingly for Alter

Of course, all real cases are mixed cases, and according to the theorem on normative emergence above, cognitive interaction will sooner or later become more and more normative. Without empirical data we see no reason to use the words "very soon", however (no. 4 above). For all interactions, then, four kinds of expectations are needed (two with Ego as a subject, two with Alter as a subject) and this is the famous pattern of "double contingency both ways".

This distinction may serve as a point of departure for the analysis of a case which has always intrigued sociologists (e.g., 18 pp. 402 ff. and 10 pp. 277 ff.): the meeting between two strangers, where the extent to which they are strangers to each other is a question of degree with (perhaps) the first meeting with a Martian as the limiting point, with close to purely cognitive interaction. If there is a limit to how much dissonance a person can stand between perception and expectation, there are two possibilities if one starts out meeting the stranger with a set of *a priori* normative expectations: either one can cling to them and avoid dissonance by cutting off contacts with strangers or extreme measures of control (usually referred to as the patterns of ethnocentrism, prejudice and discrimination), or he can decrease the degree of normativity of the expectation in order to build up new norms based on inferences from more cognitive expectations (often referred to as tolerance).

2. This passage seems to bring us closer to a more fruitful usage of the term "complementary". First of all it is stated that "complementarity" does *not* mean E → A = A → E, although this certainly may be the case between social equals. But what is meant when it is said that "the actions of each are oriented to the expectations of the other"? Only what is said above: that E(A → E) and A(E → A)

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are significant, because the sentence can only have meaning when the word "perceived" is inserted before "expectations". The last sentence brings out again the importance of comparing $A(A \rightarrow E)$ with $A(E)$ and $E(E \rightarrow A)$ with $E(A)$, which by definition will give us the extent of *conformity*. But there is no reason why we shall have two terms for the same thing, when "conformity" alone can do the job.

"Conformity" thus implies a comparison between perceived action and perceived expectation and is mostly used when the expectation is normative. We suggest using "complementarity" to imply a comparison between two sets of expectations, viz., between Ego's perception of what Alter expects of him and what Alter actually does expect of him. Thus, we define:

There is *complementarity* in the interaction system to the extent that $E(A \rightarrow E)$ overlaps with $A(A \rightarrow E)$ and to the extent that $A(E \rightarrow A)$ overlaps with $E(E \rightarrow A)$. The *complementarity is perfect* when

$$E(A \rightarrow E) \triangle A(A \rightarrow E) = \emptyset \text{ and } A(E \rightarrow A) \triangle E(E \rightarrow A) = \emptyset$$

There is *conformity* in the interaction system to the extent that $E(E \rightarrow A)$ overlaps with $E(A)$ and to the extent that $A(A \rightarrow E)$ overlaps with $A(E)$. The *conformity is perfect* when

$$E(E \rightarrow A) \triangle E(A) = \emptyset \text{ and } A(A \rightarrow E) \triangle A(E) = \emptyset$$

Of course, we can have complementarity without conformity, because one thing is agreement on the expectation-level; another thing on the action-level. And, vice versa, we can have conformity without complementarity.

One fundamental difference between the two in addition to the different levels involved is that whereas conformity is decided within one individual, it takes the whole social system to decide on the complementarity. This can be done in rare cases by making the prescriptions explicit (as in legal cases, when the defendant argues: "I thought the law said —"); in general it is done by inferences from patterns of nonconformity. If deviance is persistent and does not yield to sanctions a possible explanation may always be that there is lack of complementarity in the expectations. In such cases, an outside observer or "interaction-supervisor" may be of great help in *finding out whether there is a problem of conformity or a problem of complementarity*. It seems that our culture is more lenient towards

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people who have perceived expectations wrongly than towards people who act wrongly, because the latter group "should have known better". Hence, in every socialization process there will be a transition from the interpretation of dissonance in terms of insufficient complementarity to insufficient conformity. Essentially, *socialization* seems to be definable as an effort to attain complementarity, where the socializee's lack of conformity may be excused on the basis of his (alleged) lack of insight in normative expectations. This may save the socializee, but more significantly, it may save the system from defining more acts as deviant than it can tolerate by introducing many "socializees".

The definitions of conformity and complementarity are hinging on the comparisons of pairs of sets, and equality is singled out as "the perfect case". Remembering the four other possibilities, this means that we have four imperfect cases. Let us just take case 2 for conformity:
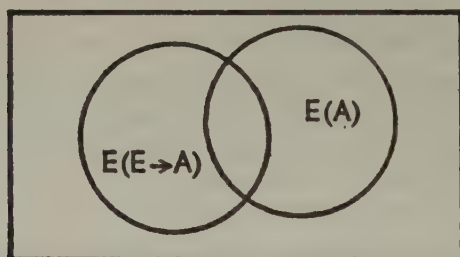
The subset to the left gives us the prescribed actions Alter did not perform (sins of omission), the subset in the middle gives us the acts of conformity, and the subset to the right gives us the proscribed actions Alter did perform (sins of commission), if we assume that the norms are of the special kind that define actions as either prescribed or proscribed (dichotomous norms).

This, however, is unrealistic, as many norms will define a set of acts that are permitted without at the same time being prescribed (i.e., indifferent) (see for instance 23, p. 4, or 1 p. 12).

We have seen that one distinction of importance to the set of comparisons of relevance to the whole expectation process is the distinction between intra-actor comparisons and inter-actor comparisons. Sociologically, the latter are by far the more interesting, as it always remains problematic how such comparisons can be done at all. One example has

been given, *viz.*, that of complementarity. Another example of equal importance is this: Ego performs the comparisons illustrated in the diagram above and concludes that there is a certain deviance present. Alter does the same. From either actor's subjective point of view a certain amount of value has been given out and a certain amount of value has been received. Some of this value can be described as *conformity-value,* with an implicit notion of reciprocity: "I conform to your norms, and you will in return be kind enough to conform to mine". Evidently, the net conformity-value for Ego is a function of

$$E(E \rightarrow A) \cap E(A) \text{ (value received) and } E(A \rightarrow E) \cap E(E) \text{ (value given)}$$

A similar comparison takes place in Alter, and in addition, they both have their hypotheses as to the comparison that goes on in the other. But how are these comparisons to be compared? This would be a suitable task for the sociological analogue of the ether concept in physics: the group mind. But instead of reverting to that or similar concepts a large variety of analytical problems present themselves; for instance *the role of the third person.* If his interaction with Alter is sufficiently similar to Ego's, Ego may use his reactions as a model for his own behavior, and for instance interpret them as signs that he finds the conformity-value received from Alter insufficient. Of course, Ego can himself conclude that he gets more value than he receives and be content, but he cannot alone conclude that his net balance is better than Alter's net balance.

* *

At this point we choose to stop, and devote the remaining lines to some reflections on what we think we have done. Evidently, it has to do with what J. H. Woodger refers to in his *The Technique of Theory Construction* (22, pp. 65 ff) when he distinguishes between four processes that may enter in the construction of scientific theories: *formalization, axiomatization, logical analysis* and *symbolization.* The latter he defines as (*ibid.,* p. 66): "the use of single letters or other printed shapes or of small groups of such in the place of ordinary words."

In our attempt to clarify the concept of 'expectation' a logical analysis has been made. In developing the higher order expectations, however, it soon became clear that natural languages were insufficient because of the complexity and clumsiness of the expressions involved. Hence the necessity of symbolization, in Woodger's sense.

The combination of analysis and symbolization made possible an *explication* in Carnap's sense of some central passages on interaction.

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It is believed that this discussion, and particularly the distinction between normative and cognitive interaction and the theorem on the emergence of norms has brought this discussion a step further.

However, were the symbols really necessary? Perhaps not for the logical analysis, but they were necessary at least to the present author for the explication of Riesman's concepts, the concepts of complementarity and conformity and for the definitions of the two kinds of interaction. But were they necessary only in the same sense as a scaffolding is necessary; as a heuristic device which can be discarded once the construction is finished?

One answer to this is indicated: *the symbols generate problems.* For a given interaction process, simple combinatorics will give the analyst a complete list of orientations *within this system of analysis,* and it would be strange if this should not strike a chord. Of course, it would be just as wrong completely to discard it as it would be to suggest this as a method of scientific innovation. For one thing, if the concept of 'comparison' is accepted, the symbols will immediately ask of the reader: *where* and *how* does this comparison take place?

But usually it is requested of symbols to yield more, though I feel that their use is already justified in the present context. The two words "formalization" and "axiomatization" lead us towards the ideas of a "calculus" and an "artificial language", more specifically a calculus of interaction processes. As everybody working in this field knows, the construction of such a calculus that satisfies formal requirements and yields nontrivial sociological insights as well is extremely difficult. It may be said that the present symbols can be used in a class calculus, but it seems doubtful that this will yield anything more than a paraphrase over the words "not", "or" and "and". Somehow empirical insight, (particularly concerning how the outcome of one comparison is fed into the next,) must be brought into the system and used in axiomatic form to reduce the number of possible symbol-strings, or some other kind of decision process may be used to decide whether a "string" expresses any social reality or not. This is useful activity when the complexity of the concepts and their interrelations transcends our usual capacity so that we must resort to a mechanical device like a formalism to get what is inherent in the concepts and the axioms. Nothing of this kind is presented in this article, but we hope to take steps in this direction in a later article.

However, we would tend to agree with Smart (19, pp. 237 ff.) that natural languages have positive features artificial languages do not

possess, exactly because of their ambiguity and richness; and that an artificial language "has its roots in ordinary language and — — draws almost its whole life from ordinary language" (*ibid.*, p. 224). In a sense an artificial language can only provide us with a "fossilized" theory limited by the limits of the artificial language itself, but that this should be a theory "with no surviving scientific purpose" (*ibid.*, p. 237) is hard to see for the reasons mentioned in the section above. But at any rate it seems clear that success in this field is inversely related to the size of the pretended field of application, as Fenstad points out (4, pp. 35 ff.) when he characterizes as a failure "the pretentious program of *total application*". Intuitively we feel that the slice we have cut off the total cake in this article is of a reasonable size for formalization.

But this is an article about the role of expectations in interactions and not about the merits or demerits of formalization. Essentially, it is an effort go further along one of the many paths suggested by Talcott Parsons. And here we are considerably at variance with Herman Tennessen (21, p. 195) when he gives his "awful warning" against The Social System by Parsons. Apart from its considerable merits in guiding *sociologists* in their empirical and theoretical work, Parsons' works have one virtue which should rank high also by Tennessen's standards: an audacity unheard of in a jungle of small-scale empirical platitudes. It may well be it will all be torn down sooner or later, but that is the fate of all science — — and in the process we suggest that more valuable sociology will ensue than from many other activities just because of the challenge.

REFERENCES

1. ANDERSON, A. R. and MOORE, O. K.: "The Formal Analysis of Normative concepts", *American Sociological Review*, 1957, no. 1.
2. CARTWRIGHT, D. and HARARY, F.: "Structural balance: A Generalization of Heider's Theory." *Psychological Review*, 1956.
3. FENSTAD, J. E.: "Notes on the Application of Formal Methods in the Soft Sciences." *Inquiry*, 1959, no. 1.
4. FESTINGER, L.: *Theory of Cognitive Dissonance*, Row Peterson 1957.
5. GALBRAITH, J. K.: *The Affluent Society*, Houghton Mifflin, 1958.
6. GOFFMAN, E.: *The Presentation of Self in Everyday Life*, University of Edinburgh, 1956.
7. HEIDER, F.: "Attitudes and cognitive organization", *Journal of Psychology*, 1946.
8. HEIDER, F.: *The Psychology of Interpersonal Relations*, Wiley 1958.
9. NAESS, ARNE: "Do we know that basic norms cannot be true or false?" *Theoria*, 1959, no. 1.

10. NEWCOMB, T.: "The Study of consensus," in Merton R. K. et al. eds.: *Sociology Today*, Basic Books 1959.

11. OFSTAD, H.: "The Functions of Moral Philosophy," *Inquiry*, 1958, no. 1.

12. OSGOOD, C. E. and TANNENBAUM, P. H.: "The Principle of Congruity in the Prediction of Attitude Change," *Psychological Review*, 1955.

13. PARSONS, TALCOTT: *The Social System*, The Free Press, 1951.

14. PARSONS, TALCOTT et al.: *Towards a General Theory of Action*, The Free Press, 1951.

15. QUINE, W. O.: *From a Logical Point of View*, Harvard University Press, 1953.

16. RIESMAN, DAVID: *The Lonely Crowd*, Doubleday, 1955.

17. ROSENBERG, M. and ABELSON, R.: "Symbolic Psycho-Logic: A Model of Attitudinal Cognition." *Behavioral Science*, vol. 3, no. 1.

18. SIMMEL, GEORG: "The Stranger," in Wolff, K. H. ed.: *The Sociology of Georg Simmel*, The Free Press, 1950.

19. SMART, J. J. C.: "Theory Construction," in Flew, A. G. N. ed.: *Logic and Language*, Basil Blackwell, 1953.

20. SULLIVAN, H. S.: *The Interpersonal Theory of Psychiatry*, Norton, 1953.

21. TENNESSEN, H.: "On Worthwhile Hypotheses," *Inquiry*, 1959, no. 2.

22. WOODGER, J. H.: "The Technique of Theory Construction," *Int. Enc. of Unified Science*, vol. II, no. 5.

23. WRIGHT, G. H. von: "Deontic Logic," *Mind*, 1951, no. 1.

THE DOUBLE AWARENESS IN HEIDEGGER AND WITTGENSTEIN

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I.

Heidegger and Wittgenstein are naturally to a high degree philosophical contraries. The aim of this article is not to deny this. But in spite of the difference in intention and philosophical attitude there is a similar awareness (consciousness) in both. The purpose of the article is to show this.

This special awareness is based on a particular experience. (The term "awareness" is used as a translation of the Swedish "medvetenhet", and "consciousness" as a translation of "medvetande".) This experience can preliminarily be defined as an experience of the world's "that", not only of its "what" or "how". This awareness is here called "that" — consciousness or the double awareness ("— — Sein besagt: Anwesen des Anwesenden: Zwiefalt." ("Being means: Presence of the Present: Duality.") (*Vorträge und Aufsätze*, 252).

II.

Heidegger's philosophy is primarily ontology.

For this reason he distinguishes between "*Being*" and "*What-is*". The *ontological* awareness relates to "Sein", the *ontical* to what is "Seiendes".

What "Seiendes" is, is not hard to grasp. Everything that is, of whatever kind it be, the empirical given or ideas or imaginings, everything is "Seiendes", that of which I am ontically conscious.

In Wittgenstein (*Tractatus*) this "Seiendes" corresponds to: How the world is (6.432; 6.44), that about which one can talk.

But what then is "Sein" when everything is "Seiendes"? It is, according to Heidegger, "dass Seiendes *ist*". ("that what-is *is*") (*Einführung in die Metaphysik*, 24; *Holzwege*, 53, 54). There is something and nothing is not, says Heidegger. It is this mystical (in Wittgenstein's sense) "Sein" which the ontological analysis will clarify. The question of "Sein" is formulated by Heidegger thus: "Warum is überhaupt Seiendes und nicht vielmehr Nichts?" ("Why is there any being at all, why not far rather Nothing?") (*Einführung in die Metaphysik*, I; *Was ist Metaphysik?*, 21, 23.)

The "that"-consciousness is not a consciousness of something *in* the world, but a consciousness *that* we are, that something exists, etc. Not a consciousness of the content of the world, but *that* there is content in this world. "Diesen in seinem Woher und Wohin verhüllten, aber an ihm selbst um so unverhüllter erschlossenen Seinscharakter des Daseins, dieses 'Dass es ist' nennen wir die *Geworfenheit* dieses Seienden in sein Da, so zwar, dass es als In-der-Welt-sein das Da ist. Der Ausdruck Geworfenheit soll die *Faktizität der Überantwortung* andeuten. Das in der Befindlichkeit des Daseins erschlossene 'Dass es ist und zu sein hat' ist nicht jenes 'Dass', das ontologisch-kategorial die der Vorhandenheit zugehörige Tatsächlichkeit ausdrückt. — *Faktizität ist nicht die Tatsächlichkeit des factum brutum eines Vorhandenen, sondern ein in die Existenz aufgenommener, wenngleich zunächst abgedrängter Seinscharakter des Daseins.* Das Dass der Faktizität wird in einem Anschauen nie vorfindlich." ("This disclosed character of Being of the Being-there which is veiled in its whereform and whereto but in itself even more unveiled, this 'That it is' we call the throwness of the Being-there into its there but so that it, as a Being-in-the-world, is the there. The term 'throwness' is to signify the '*Faktizität*' *of the responsibility*. What is disclosed in the 'Befindlichkeit' of the Being-there, namely 'that it is and has to be' is not that particular 'That', which ontologically-categorically expresses the Being-at-hand, which belongs to 'Tatsächlichkeit'. — '*Faktizität*' *is not the 'Tatsächlichkeit' of the factum brutum of a Being-at-hand but a character of Being of the Being-there which is absorbed into the existence although at first it seems put aside.* The 'that' of the 'Faktizität' is never at hand in an experience.") (*Sein und Zeit*, 135). Heidegger refers to this "Seinscharakter des Daseins ("character of Being of the Being-there") with the word "Faktizität". "Sein" is not something "Seiendes" but "dass Seiendes *ist*." ("that what-is *is*"). Experience can tell how something (or everything) "Seiendes" ist, but not *that* something is. For this a special awareness is required. Wittgen-

stein says: "— that something *is;* but that is no experience." (5.552).
The awareness of "Sein", that something is, is not a consciousness of
something.

This fundamental question of metaphysics is as important to Wittgen-
stein as to Heidegger. The philosophical "atmosphere" is similar in
Wittgenstein and Heidegger, and the reason is that they are conscious
not only of *how* the world is but also *that* it is. (6,44). In order to be
aware of this, a special experience of the world is necessary: an experience
and consciousness of the nothingness of the world and an experience
of man as thrown out in this nothingness (A kind of counterpart to the
difference between experience and consciousness in Heidegger is found
in a passage from "Holtzwege": "Wieder eine andere Weise, wie Wahr-
heit zum Leuchten kommt, ist die Nähe dessen, was schlechthin nicht
ein Seiendes ist, sondern das Seiendste des Seienden. — Wieder eine
andere Weise, wie Wahrheit wird, ist das Fragen des Denkens, das als
Denken des Seins dieses in seiner Frag-würdigkeit nenne." (*Holzwege,*
50). ("Yet another way by which truth appears is the closeness of that
which as a matter of fact is not a what-is but the Most-is of what-is.
— Yet another way for truth to become manifest is the questioning of
Thought which, as the Thought of Being, names this in its question-
evaluation.").) Such an ontical-existential experience is the basis of
Heidegger's ontological-existentialistic talk about "das ursprüngliche
geworfene In-der-Welt-sein als Un-zuhause, das nackte 'Dass' im Nichts
der Welt." ("the originally thrown Being-in-the-world as not-at-home,
the pure 'That' in the Nothing of the world.") (*Sein und Zeit,*
276—7).

H. *Jonas* has interpreted the gnosticism as being based on such a
fundamental experience of the world. (*Gnosis und spätantiker Geist*).
The reason why he has been able to interpret the gnosticism according
to Heidegger's philosophy with such an ease and success is that the
gnostic conception of the world is very similar to that of Heidegger.
Sein und Zeit is a "gnostic" work. (Heidegger could say that *Sein und
Zeit* is the ontological comprehension of the ontical experience of the
gnostics; *Sein und Zeit,* 201). But Wittgenstein's (*Tractatus*) theology
is also gnostic: "God does not reveal himself *in* the world." (6.432).
To Wittgenstein the idea that God could be the Creator of the world
was completely unintelligible. The same attitude is expressed in Wittgen-
stein's rejection of pantheism. "To say that Wittgenstein was not a
pantheist is to say something important." (von Wright, *The Philosophical
Review* 1955, p. 543). Likewise gnostic cosmology and anthropology

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are manifest in von Wright's picture of Wittgenstein: "Wittgenstein had the conviction, he sometimes said, that he was doomed. His outlook was typically one of gloom. Modern times were to him a dark age. His idea of the helplessness of human beings was not unlike certain doctrines of predestination." (*The Philosophical Review*, p. 543). (Cf. Heidegger's definition of "Dasein" as guilt, *Sein und Zeit*, 284 ff.)

The experience of the *fundamental question of metaphysics and this "gnostic" conception of the world* are intimately bound together. The fundamental metaphysical question brings us before "Sein", but this can only be done in such a way that the "Seiende" shows its nothingness (the "gnostic" vision of reality). The metaphysical question — about "Sein" — arises only when we first have experienced the "Seiende" as nothingness. Interpreted *à la* Heidegger: the ontological understanding has an ontic condition. (See the conclusion of this article!)

"Warum ist überhaupt Seiendes und nicht vielmehr Nichts? — Jeder wird einmal, vielleicht sogar dann und wann, von der verborgenen Macht dieser Frage gestreift, ohne recht zu fassen, was ihm geschieht. In einer grossen Verzweiflung z. B., wo alles Gewicht aus den Dingen schwinden will und jeder Sinn sich verdunkelt, steht die Frage auf. — In einer Langeweile ist die Frage da, —, wo aber die hartnäckige Gewöhnlichkeit des Seienden eine Öde ausbreitet, in der es uns gleichgültig erscheint, ob das Seiende ist oder ob es nicht ist, — (*Einführung in die Metaphysik* I). ("Why is the what-is at all and not rather Nothing? — Everybody is once, perhaps even at times, faced with the secret power of this question without really understanding what is happening to him. For instance, in great misery, when all importance seems to disappear from things and every meaning is obscured, then the question arises. — For quite some time the question stands there, —, but the obstinate commonplace persistence of the What-is here spreads a desertlike emptiness in which we do not care whether the What-is is or is not. —"). But even in Wittgenstein this question lies behind all other questions. Heidegger's words can be adapted to Wittgenstein: "Unsere Frage ist die *Frage* aller wahrhaften, d.h. sich auf sich stellenden Fragen und sie wird, ob wissentlich oder nicht, in jeder Frage notwendig mitgefragt." (*Einführung in die Metaphysik* 55). ("Our question is the *question* of all true questions, i.e. that is questioning the questions themselves, and it will necessarily be asked in every question, whether deliberately or not.") One can interpret Wittgenstein's philosophy as an effort to make simple that which seems enigmatic, to solve the puzzles which the philosophers have raised, and so leave everything as it was:

"It (the philosophy) leaves everything as it is." (*Phil. Invest.,* 124); yet not in order to abolish the enigmatic, but in order to put the enigmatic in the right place. In his book on Wittgenstein Malcolm quotes Wittgenstein's remark that he sometimes is struck by the wonder that something exists, that the world is. This amazement has been expressed both in Wittgenstein's earlier and in his later writings. One may say the activity of Wittgenstein as a philosopher is based on this metaphysical wonder. There is nothing enigmatic in the world. An enigmatic content does not exist. (4.003) But *that* the world exists, that is enigmatic. "We find certain things about seeing puzzling, because we do not find the whole business of seeing puzzling enough." (*Phil. Invest.* 212a). In order to purify the "that" — consciousness, Wittgenstein wants, through his philosophical work, to do away with the feeling of amazement — a kind of giddiness — when faced with philosophical and mathematical statements; the proper amazement has to be reserved for the world's "that". *This* amazement cannot be dissolved, but amazement concerning the content of the world can be dissolved through the philosophical critique of language.

"What *we* do is to bring words back from their metaphysical to their everyday usage," (*Phil. Invest.,* 116. Also 118, 119, 126, 133, 196 and other places). The aim of the *Tractatus* is to show what is important by making all that can be said, all facts in the world, unimportant.

Wittgenstein is closely tied to the fundamental metaphysical question. "I believe that a certain feeling of amazement that anything should exist at all was sometimes expressed by Wittgenstein, not only during the *Tractatus* period, but also when I knew him." (N. Malcolm: *Ludwig Wittgenstein, A Memoir,* p. 70.) But this question is connected even by Wittgenstein with the "gnostic" experience of the frightening mysteriousness of the world. Wittgenstein was not unfamiliar with the dread. His philosophical therapy did not make the dread disappear, but instead it can be said to have led to a fixing of the dread. The dread, according to Heidegger, is especially caused by the fact that: "Die Welt, worin ich existiere, ist zur Unbedeutsamkeit herabgesunken, —" (*Sein und Zeit,* 343). ("The world in which I exist has sunk into unimportance.") This Wittgenstein makes clear in the *Tractatus:* the world, what can be said, is worthless. "The sense of the world must lie outside the world." (6.41). The World is a desert without the oasis of values. Wittgenstein presents a world of nothingness, and thus a world of dread. If one interprets the Wittgenstein of *the Tractatus à la* the Witt-

genstein of the *Phil. Invest.:* Wittgenstein's "form of life" with its "gnostic" experience of the nothingness of the world has determined his language and what he lets go for truth: the work *Tractatus.* Interpreted *à la* Heidegger: the question of "Sein", the fundamental question of metaphysics, was raised even to Wittgenstein through the revelation of the Nothing, "das Nichts". Nothing is revealed in "Dasein" by the dread. Heidegger has taken over Kierkegaard's definition of dread: dread — for Nothing. The question of "Sein" is possible only when the possibility of Nothing has been established. (*Einführung in die Metaphysik,* 85; *Platons Lehre von der Wahrheit,* 114). Nothing is not for Heidegger an hypostasis of the negation. "Das Nichts ist ursprünglicher als das Nicht und die Verneinung" (*Was ist Metaphysik?* 28). ("The Nothing is more original than the Not and negation.") "Entgegenhandeln", "verabscheuen", "verbieten", "entbehren" ("to counteract", "to detest", "to forbid", "to be deprived of") are more important and more characteristic forms of "das Nichten des Nichts" ("the negativing function of the Nothing") than the negation of thoughts. (*Was ist Metaphysik?* 37). It is characterized by "Unheimlichkeit" ("Uncanniness"), which is hidden for us in our daily "In-der-Welt-Sein" (*Sein und Zeit,* 277) but which is revealed for us through the Nothing. "Sein": "dass Seiendes ist" is revealed in "Dasein" in the dread "Dasein heisst: Hineingehaltenheit in das Nichts" ("Being-there means: being projected into the Nothing") (*Was ist Metaphysik?* 35; cf. even 12 and 32 and *Einführung in die Metaphysik,* 135). The amazement which makes Wittgenstein and Heidegger raise the fundamental metaphysical question is roused when "die Befremdlichkeit des Seienden uns bedrängt" ("the strangeness of the What-is forces itself upon us"), and this happens by the revelation of the Nothing through the dread. That is why the "Dasein" determined by dread is the only "Seiende", which in itself is ontological and not only ontical: it is related to "Sein" (Heidegger), to the world's "that", not only its "how". (Wittgenstein).

Man, who by his essence (as "Dasein") belongs to "Sein" is "das Unheimlichste" ("the highest uncanniness") (*Einführung in die Metaphysik,* 113 ff.). Man is the metaphysical being, is oriented towards "Sein", and the fact that he is so oriented is "das Unheimlichste". The original in Heidegger's ontology is that, in order to interpret the most general of all concepts, "Sein", he uses terms of a very special kind, taken from a "gnostic" conception of reality (dread, dead, decay, guilt, silence, conscience etc.).

To characterize the double awareness is a difficult task. Difficult in a qualified way: its object "Sein" is beyond all "Seiendes", is something similar to the One in Plato and the Neo-Platonists, is inaccessible. We stand before the unconditional abyss. (*Vorträge und Aufsätze,* 179). No reason can be given for the phenomenon that language-games are played, for all distinctions are given *within* the language-game. (Wittgenstein, *Phil. Invest.) That* the world is, is the mystical. (Wittgenstein, *Tractatus*.) It cannot be expressed in sentences; it is the awareness of that which is higher, that about which nothing can be said. (Wittgenstein.) It is awareness of "Sein", not consciousness of anything (or all) "Seiendes", and can only be attained by the "Denken" ("thinking") which has affinity with "Dichten") ("poetry"), not by scientific thought or by habitual philosophizing. (Heidegger.) (*Platons Lehre von der Wahrheit,* 54; *Vorträge und Aufsätze,* 192; *Einführung in die Metaphysik,* 131; *Was ist Metaphysik?* 50—51). Its verb is "show", not "say". (Wittgenstein, *Tractatus*). Even Heidegger uses the verb "zeigen" in this connection.

1.

Awareness is to be lifted from the low-land of the habitual conceptions to see that which is invisible for ordinary eyes: become alive to something long known. Then "I become alive to" what it means to be a human being. In the "that"-consciousness I see everything as *new*. Thinking on the level of the matter-of-fact consciousness makes everything old, puts everything back in the medium of the past (to talk with Kierkegaard). "Nothing is new under the sun."

2.

Characteristic of the double awareness is its contemplation of the *whole* (*Einführung in die Metaphysik* 2), Heidegger refers to Plato's and Aristotle's distinction between whole and sum, between ὅλον and πᾶν (*Sein und Zeit,* 244, the note). This concerns not a quantitative but a, so to say, qualitative whole. "The contemplation of the world *sub specie aeterni* is its contemplation as a limited whole." (6.45) The Philosophy of the *Tractatus* gives this contemplation *sub specie aeterni*. The philosophy shall from within the unthinkable circumscribe this through the thinkable (4.114). By clearly stating what can be said,

philosophy shall "mean" that which cannot be said. Such an activity is possible only *sub specie aeterni,* whereby the world appears as a limited whole. And thus the question is about a "qualitative" whole. Wittgenstein does not give a complete description of the world; that is given in the sum of all true elementary statements. (4.26). It is, even for Wittgenstein, a question not about the sum but about the whole, not about the "Seiende" but about "Sein", not about an ontical, but about an ontological whole. This contemplation of the whole is not possible for the matter-of-fact-consciousness, but only for the "that"-consciousness.

It is impossible for Wittgenstein to express in language this whole of the world[1]. What is an impossibility for Wittgenstein is for Heidegger a difficulty. Wittgenstein does not mean, however, that such a *conception* of the whole is impossible, but only that it cannot be expressed. According to Wittgenstein metaphysics is an impossibility as statements, as system, but not as awareness. On the contrary he presupposes the possibility of the metaphysical consciousnes. "There is indeed the inexpressible" (6.522). By denying the possibility of metaphysical statements Wittgenstein thus affirms the possibility of the metaphysical consciousness and also assigns to it the predicate The Higher. Thus he indicates its priority over the world of language. (In *Phil. Invest.:* "form of life" determines the "language-game").

3.

Heidegger, who does not have Wittgenstein's (*Tractatus*) narrow ideal of language, says much about the characteristics of the real existence. Real existence, being "Sein zum *Tode*" ("the Being towards one's *death*") is conceived as a whole, although this conception of the whole is an impossible possibility, as death is an impossible possibility for life (*Sein und Zeit,* 236, 265—266). *Possibility,* because we could not at all know the distinction between real and non-real existence, if it were not a possibility. "Sein zum Tode" is possibility to authentic existence. *Impossible* possibility: it is impossible to conceive "Dasein" as a whole, because "Dasein" is by nature "sich vorweg." (*Sein und Zeit,* 20, 236). ("in-advance-of-itself").

It is an impossibility in a categorical respect (as "Vorhandenes") ("What-is-at-hand"), but possibility, in an existentialistic respect (as "Dasein"). As "Dasein" is always "'mehr' als es tatsächlich ist", ("As "Being-there" always is "'more' than it really is"), it is an

impossibility, whereas it is a possibility, as it is "nie mehr, als es faktisch ist" ("never more than it actually is"). Hence, the command is a right one: "'werde, was du bist!'" (*Sein und Zeit*, 145). ("'become what thou art!'")

In Wittgenstein: "Death is not an event of life. Death is not lived through." (6.4311). It is man in his unreal existence who conceives death as an event, whereas death for the real existence is its utmost possibility: "Der Tod ist die Möglichkeit der schlechtinnigen Daseins-unmöglichkeit." (*Sein und Zeit*, 250). ("Death is the possibility of the outright impossibility of Being-there.") Both for Heidegger and Wittgenstein the question about death is a question of the double awareness. For the simple matter-of-fact-consciousness the problem of death is a feigned problem. The existentialistic analysis of death is *before* all biological, psychological or theological questions of death. (*Sein und Zeit*, 248). Death belongs, like, for example, the self, to the metaphysical, and limits the world from the other side, one might say. But philosophy, the philosophy of the double awareness, is within the sphere of the unthinkable. "It should limit the unthinkable from within through the thinkable." (4.114). "It will mean the unspeakable by clearly displaying the speakable" (4.115).

The task of philosophy is to stand within the unthinkable and by clearly depicting the thinkable, point to the unthinkable (Wittgenstein). Being an expression of the real existence, the task of philosophy is to clarify the unreal existence and thereby express the real existence as a possibility (Heidegger.) "Sein zum Tode" is the "Ganzseinkönnen" of man ("'whole' potentiality of Being") (*Sein und Zeit*, 266, 326). Both Heidegger's and Wittgenstein's philosophy stand within the unthinkable, and both point out the metaphysical self and death as the two limits within the unthinkable, between which the world is. Because both Heidegger and Wittgenstein are aware of these boundaries, they can contemplate the world as a whole. Their contemplation of the whole (of the world as a limited whole) is existentialistic metaphysics (the philosophical ego, i.e. the thinker who carries on the philosophical activity, is the metaphysical ego). The philosophy of Heidegger and Wittgenstein is the philosophy of the double awareness.

4.

In this "that"-consciousness, the *ego* is not an object. (5.641). It is in relation to the whole life of a human being that the different

characters of those two levels of consciousness become apparent. The simple matter-of-fact-consciousness does not put me under an obligation, and has nothing to do with my moral life. The matter-of-fact-consciousness does not change anything in any personality. I may have this matter-of-fact-consciousness even with myself as object. Then I am an object among other objects and thereby exposed to the same laws as other objects. Hume, who denies the validity of the concept of substance, is right in doing the same thing with the ego — on the level of the matter-of-fact-consciousness. As a thing among other things the ego is not anything *sui generis*. *Only through the double awareness* is it elevated to its *unique position*.

"Die Person ist kein Ding, keine Substanz, kein Gegenstand." *(Sein und Zeit* 47; see even *Sein und Zeit,* 332) ("The person is not a thing, not a substance, not an object"). This I, the metaphysical subject, is, according to Wittgenstein, the philosophical subject. And philosophy is to him an *activity,* something which is done by somebody. "(Words are also deeds)" *(Phil. Invest.,* 546). "Philosophy is not a theory but an activity". (4.112). This implies that what is not linguistic in the activity of language, the one who speaks, the I, does not belong to the sphere of language, to the world. The I is not at home in the world, although the world is always my world. The I is an a-cosmic quantity. According to Heidegger: "Person" is "Aktvollzieher" ("someone accomplishing acts") and "Akte sind etwas Unpsychisches" ("acts are something unpsychical"). "Psychisches Sein hat also mit Personsein nichts zu tun." ("Psychical being has thus nothing to do with being a person") *(Sein und Zeit,* 48) (See III 7!).

This distinction between "Tatsächlichkeit" und "Fakticität" can clarify what Wittgenstein means when he says that the subject does not belong to the world, but is a limit of the world (5.632). "There is therefore really a sense in which in philosophy we can talk of a non-psychological I.

The I occurs in philosophy through the fact that 'the world is my world.'

The philosophical I is not the man, not the human body or the human soul of which psychology treats, but the metaphysical subject, the limit — not a part of the world." (5.641). The philosophical I (which carries on the philosophical activity) is the metaphysical I; it stands within the unthinkable and does not exist in the "Tatsächlichkeit" of experience but only in relation to the double awareness, which is aware of the "Faktizität."

The philosophical I, which carries on the philosophical activity, is, according to Wittgenstein, the metaphysical subject and does not belong to the content of the world. "The subject does not belong to the world but it is a limit of the world." (5.632). (*Phil. Invest.*, 404, 405, 410). Heidegger says: "Der 'Geist' fällt nicht erst in die Zeit, —" (*Sein und Zeit*, 436). ("The 'Spirit' is not at first in time"). Wittgenstein does not forget that he exists, when he thinks. The subjective thinker himself is the meeting-place for the intellectual (the language, the world) and the mystical, the higher, the meaning of the world. The subject when considered as an empirical fact, as an I, is not a subject. The subject is not the objective, fixed point (in Descartes' way). It exists, not for the matter-of-fact-consciousness, but for the "that"-consciousness. The metaphysical I is the invisible-point which is the union between the thinkable and the unthinkable, between the world of language and the higher. It belongs itself to the unthinkable, and is not a part of the world as the eye is not a part of the field of vision. (5.632, 5.633, 5.6331). Here is the place of wonder, where the metaphysical amazement arises: *that* something exists. Wittgenstein clears away all false puzzles about the content of the world in order to make room for this "real" puzzle: The fundamental metaphysical question. In the same way Heidegger turns against all traditional metaphysics, because its question is not of "Sein" but of "Seiende"; the traditional metaphysics answers the question of "what", but not the question of "that". The higher relates to the world only at this point, in the metaphysical I, and not elsewhere. *This* solipsism is the philosophical attitude. That something *is,* is not an experience: does not belong to the content of the world. (*Tractatus*). The real object of wonder is what is given and presupposed, what ordains the language-game, what tells what is the truth, what determines the essence of things (*Phil. Invest.*). In the "object" of wonder is included the metaphysical subject itself, to whom this amazement is given. (*Vorträge und Aufsätze*, 71 ff., 234 ff., *Einführung in die Metaphysik*, 136, 155; *Platons Lehre von der Wahrheit*, 50, 65). The metaphysical wonder is possible only if the philosophical subject is drawn into the realm of the unthinkable and itself belongs to the mystical. Wittgenstein's metaphysics is existential: the thinker, who carries on the philosophical activity that implies the unveiling of the limits of the thinkable, stands himself beyond the limits of the thinkable. Interpreted in Heidegger's way: the fundamental metaphysical question, the question of "Sein", is given only in "Dasein". It is only when "das Nichts" reveals "Sein" in "Dasein" that

one asks "Warum ist überhaupt Seiendes und nicht vielmehr Nichts?" ("Why is the What-is at all and not rather Nothing?") (Heidegger). This is the question which places philosophy within the unthinkable and thereby starts the philosophical activity: to point out the limit of the thinkable (Wittgenstein, *Tractatus*). Philosophy is, as a language-game, determined by and presupposes the given, "forms of life", which ordain what the language can say. That the language-game is played is the fundamental phenomenon (*Phil. Invest.*, 654) that arouses our "real" metaphysical wonder that something is given, that which is presupposed in all language (Wittgenstein, *Phil. Invest.*).

Wittgenstein is a mystic; he is silent about the higher. He lets metaphysics remain mystical, and his metaphysics has the minimal form of the mystic. His *method* is a *via negativa* but thereby he has not denied but affirmed "das Höhere", and he stands with his philosophy (*Tractatus*) within the unthinkable and the inexpressible. (4.114; 4.115). Heidegger, on the contrary, chooses an ideal of language according to which even statements about "das Höhere" of Wittgenstein are meaningful (and meaningful *par preference*). Heidegger and Wittgenstein differ on the question of ideal of language, but what they wish to show is basically the same.

5.

The "*existential*" *character* of the double consciousness is due to its being a consciousness of the qualitative whole. (The word 'existence' is here not taken in Heidegger's sense. Especially in his later philosophy Heidegger interprets the concept as an ontological concept (*Platons Lehre von der Wahrheit*, 66 ff.). The word is here instead taken in its merely existential sense, implying that Heidegger's philosophy is regarded as dependent upon Heidegger's existential experience). In accordance with our programme (see part IV) Heidegger's ontological interpretation is traced back to its ontic-existential point of departure. This is done because the conception of the whole found in the double awareness implies that the thinker himself is drawn into thinking in terms of the double awareness. "Die Frage nach dem Nichts (only "das Nichts" can in "Dasein" reveal "Sein", which is the "object" of the "that"-consciousness) stellt uns — die Fragenden — selbst in Frage. Sie ist eine metaphysische." (*Was ist Metaphysik?*, 41). ("The enquiry into Nothing puts us, the enquirers, ourselves in question. It is a metaphysical one."). In the same way Wittgenstein is conscious that he is buried in his own philosophy. This is shown by the fact that Wittgenstein in the

Tractatus at the end denounces even his own Philosophy as developed in the *Tractatus* (because there can be no philosophical statements); in *Phil. Invest.* he indicates the "Weltanschauung" that is the base of what he expounds in this work. His own philosophy depends on the form of life which he has chosen. "The concept of a perspicuous representation is of fundamental significance for us. It earmarks the form of account we give, the way we look at things. (Is this a 'Weltanschauung'?)" (*Phil. Invest.*, 122).

In our double awareness we have responsibility. Ethics, which is transcendental, sets, according to Wittgenstein, the limits of our world. It cannot change anything *in* our world, but it can change its limits. About the meeting between the unthinkable and the thinkable Wittgenstein says: "the 'world is my world'." (5.641). Neither for Wittgenstein nor for Heidegger is the subject ever separated from the world: "*Im Ich-sagen spricht sich das Dasein als In-der-Welt-sein aus.*" (*Sein und Zeit*, 321); ("In saying-I the Being-there expresses itself as Being-in-the-world."); "*eigentliches Selbstsein*" ("authentic self-Being") is nothing but "*In-der-Welt-sein*" (*Sein und Zeit*, 298, 13). This is not anything that can be said (Wittgenstein), nothing that belongs to "das Vorhandene" (Heidegger). It can only be shown (Wittgenstein); it belongs to "das Dasein", to the existentialistic (Heidegger). This existential relationship between the world of language and the mystical cannot be expressed, says Wittgenstein (in that case it would be within the world of *language*). It does not belong to "die Tatsächlichkeit" but to "die Faktizität." There is no question about a "*factum brutum eines Vorhandenen*", ("*factum brutum* of a Being-at-hand"), but instead about "Dass es ist" ("that it is"); "Dasein" is what is concerned, Heidegger tells us. (*Sein und Zeit*, 135).

For Wittgenstein the limit of language and the limit of the world coincide (5.6; *Phil. Invest.* 371, 373). What this implies becomes apparent in a parallel passage in Heidegger; the existential implications of Wittgenstein's statement are expressed by Heidegger as follows: "*Erkennen ist eine Seinsart des In-der-Welt-seins*" (*Sein und Zeit*, 61). ("To know is a kind-of-Being of the Being-in-the-world.") The concepts are not created by the constructive thinker but force themselves on us. "A *concept* forces itself on one. (This is what you must not forget.)" (*Phil. Invest.*, 204e). This power of the concepts to force themselves on us is due to the fact that they are founded in life. It is our attachment to life that attaches us to concepts. "What people accept as a justification — is shown by how they think and live." (*Phil. Invest.*,

325). Which are the concepts that become ours therefore depends on our life, on our world as a whole. And it is this whole world which is put into question in Wittgenstein's and Heidegger's philosophy, not as something but as "that". A sign of this is that the thinker is not unaffected by his philosophy. The thinker, the acting subject, (the metaphysical subject according to Wittgenstein) participates in Heidegger's and Wittgenstein's "mystical" philosophy (in Wittgenstein's philosophy, since the philosophy is within the realm of the inexpressible and the unthinkable, it can comprise the metaphysical subject).

6.

A very important difference between Wittgenstein and the logical empiricists is, as will be known, connected with how *Wittgenstein's last sentence in Tractatus* is to be interpreted. Wittgenstein is aware of there being things of which one cannot talk. The logical empiricists mean that whereof one cannot speak, thereof one must be silent, but thereby one is not silent *about* anything (Neurath). "What we cannot say, we cannot say, and we cannot whistle it either" (Ramsey, *Revolutionen i Filosofien,* Stockholm 1957, p. 77). Wittgenstein's view is clarified in, for example, 6.522: "There is indeed the inexpressible. This *shows* itself; it is the mystical."

There are corresponding passages in Heidegger. Even Heidegger is silent *about* something. The silence which is not silence *about* something is not silence. "Schweigen heisst aber nicht stumm sein. Der Stumme hat umgekehrt die Tendenz zum 'Sprechen'. Ein Stummer hat nicht nur nicht bewiesen, dass er schweigen kann, es fehlt ihm sogar jede Möglichkeit, dergleichen zu beweisen. Und so wenig wie der Stumme zeigt einer, der von Natur gewohnt ist, wenig zu sprechen, dass er schweigt und schweigen kann. Wer nie etwas sagt, vermag im gegebenen Augenblick auch nicht zu scrweigen. — Der Mensch zeigt sich als Seiendes, das redet." (*Sein und Zeit,* 164—165) (*Platons Lehre von der Wahrheit,* 92; *Sein und Zeit,* 273). ("To be silent does not mean to be dumb, though. A dumb person has not only not proved that he can be silent, but he doesn't even have the possibility of proving any such thing. And a person who by nature is accustomed to speak little shows that he is silent and is able to be silent no more than does the dumb person. He who never says anything in thus not able to be silent at a given moment. — The human being shows himself as a what-is that speaks.").

According to this interpretation of Wittgenstein, it is fallacious and

against Wittgenstein's intentions to seek, as Russell does (in the Introduction to the *Tractatus*), to evade Wittgenstein's qualitative distinction between "say" and "show" by appeal to a meta-theory of language (in which theory "meta" does not mean that a special kind of consciousness is established. The theory is still a theory on the level of the matter-of-fact-consciousness). Russell's "intellectual discomfort" (Introd. to the *Tractatus*, P. 22) with regard to Wittgenstein's distinction is not evidence of any discomfort at the intellectual level (although this, of course, was Russell's "unconscious" view of the matter). On the contrary the intellectual level here crosses another level (the "existential"). That Wittgenstein succeeds in saying quite a lot about the inexpressible, the mystical, Russell cannot understand, except by thinking that there must be a way out through a hierarchy of language (or something like that). This implies a way out on the level of the language, of the world, which is completely incompatible with Wittgenstein's view. Russell and Reichenbach ("*Elements of Symbolic Logic*", 1952, p. 381, note) treat of Wittgenstein's distinction as if it were a distinction between two functions on the same level, whereas for Wittgenstein it is a distinction between levels of consciousness, an intellectual and an "existential" one, between consciousness and awareness. The distinction can therefore not be done away with at the intellectual level.

Seeking to abolish this distinction is not to improve or beautify the philosophy of Wittgenstein (*Tractatus*). Instead, it is to tear down its fundamental element. The difference between Wittgenstein and Heidegger, on the contrary, may be conceived as two different ideals of language (which of course in itself is an important difference). But the fact that Heidegger says much about that whereof, according to Wittgenstein, one cannot talk, does not prevent them from doing philosophy on similar premises.

That Wittgenstein does not conceive his philosophy at the intellectual level, becomes manifest in 6.54. "In, with, and under" (to use the prepositions of the Lutheran doctrine of the Sacraments) the intellectual exposition Wittgenstein has in the end not *said* anything (6.54; 7) but he has *shown* something, and this is that whereof one cannot talk. The difference between Russell-Reichenbach (the meta-theorists of language) on the one hand and Wittgenstein on the other hand is no technical difference between the philosophers of the matter-of-fact-consciousness and the philosophers of the double awareness; it is an existential difference. Wittgenstein is a real mystic; he is silent *about* something.

The criticism which here could be raised from Heidegger's and

Wittgenstein's philosophy against analytical philosophers of the Ramsey-Neurath-Russell-Reichenbach-type is not an intellectual but an "existential" criticism. They don't fail in *something* but they have not seen *that* something is; because they have chosen to be deaf towards the claims of this awareness.

The solutions of the philosophical questions have been found to depend on an interpretation of life. The philosophical discussion no longer is theoretical but becomes "existential". Not new thoughts are demanded but a new act of awareness.

Through a new act of awareness my matter-of-fact-consciousness is elevated to be binding. Hume's sceptical question of how something that is can be made to mean that something ought to be, is not solved by increased knowledge but by an intensifying of the consciousness. That is, the consciousness which man already has is not supplanted but is chosen. But man has chosen to forget, has chosen the simple, non-committing matter-of-fact-consciousness.

7.

Common to Heidegger and Wittgenstein is the interpretation of *philosophy as therapy*. They are both "psychoanalysts" as philosophers. Philosophy is an activity — not a doctrine — which aims at unveiling hidden connections and thereby liberating. They are philosophers of "enlightenment" who practise medicine of a spiritual kind. The hidden is exposed and a spiritual illness is thereby cured. "The philosophers' treatment of a question is like the treatment of an illness." (*Phil. Invest.*, 255). Man is made free to adapt himself to reality, and this implies that he is made free to act. Heidegger's and Wittgenstein's philosophy is therapy, action, and makes man free to act, by displaying even thinking as action. Philosophy is not only something theoretical that merely concerns the theoretically contemplating man detached from life. It is, on the contrary, something which is important to the acting man. The analysis itself is action and the result of this action is that the "enlightened" are made free to act.

To Wittgenstein the use of a thought is the criterion of how it is meant. Wittgenstein understands his philosophy by applying it and he teaches others to regard the application as a criterion. The therapeutic in Wittgenstein's philosophy is that he, through his philosophy, makes others free to regard the application of a thought as criterion of how it is to be comprehended. He wants to make others understand: he

wants them to act. In the action it is shown that the understanding is based on a way of living and thinking. This is the implication of what here has been called Wittgenstein's existentialism.

Wittgenstein means, like Heidegger, that the task of philosophy is to free man from the bewitchment of the language (even if he, with his strict, ascetic ideal of language, does not feel compelled to resort to a terminology so abstruse as Heidegger's). Like Heidegger, Wittgenstein means that man gains "health" through philosophical clarification. The philosophical "sickness", a paralysis of action, is healed by the philosophical activity.

Heidegger means that his analysis of "Dasein" unveils the unreal existence, and thereby reveals the real existence. In the unreal existence the understanding of "Sein" is "verschlossen", ("closed"), whereas it is "erschlossen" ("disclosed") in the real existence, through the ontological-existentialistic analysis. The existentialistic-ontological analysis makes *conscious* what was unconscious, "verschlossen", in the unreal existence.

8.

For philosophy in general *language* is a neutral means of communication. Language is (ought to be) an organ, for the pure thought, free from all contents. Against this Wittgenstein's criticism is directed: it is a superstition to believe that the logically purified concepts are the correct concepts. "The more narrowly we examine actual language, the sharper becomes the conflict between it and our requirement. (For the crystalline purity of logic was, of course, not a *result of investigation*: it was a requirement.) The conflict becomes intolerable; the requirement is now in danger of becoming empty. — We have got on to slippery ice where there is no friction and so in a certain sense the conditions are ideal, but also, just because of that, we are unable to walk. We want to walk: so we need *friction*. Back to the rough ground!" (*Phil. Invest.*, 107). Another image which Wittgenstein uses is that often we want just the unclear photography (*Phil. Invest.*, 71). To make the concepts logically pure is to abolish the dependence of the language on its surroundings, its world, in which the language has significance; it is to abolish the situation of the language.

"What is happening now has significance — in these surroundings. The surroundings give it its importance." (*Phil. Invest.*, 583).

It was such an abolition of the situation of the language that Wittgenstein himself made in the *Tractatus* in a radical way, but he also accepted the consequences: nothing important can be said.

That people agree in the language they use is "not agreement in opinions but in form of life." (*Phil. Invest.*, 241). That the language is an expression of man's attitude towards life, that it is always "polluted" with existential suppositions, that it is never a sterile organ, is a consequence of the general picture, which the double awareness provides. Hence, the futility of the attempts of certain analytical philosophers to translate existentialists and similar philosophers into the sterile language used by these analytical philosophers. Such an objectifying, de-existentializing, neutralizing, is an impossibility. "Ein Ausdruck hat nur im Strome des Lebens Bedeutung." (N. Malcolm: *Ludwig Wittgenstein, A Memoir,* p. 93, citation from Wittgenstein). Heidegger's terrific conceptual apparatus, which exploits the tremendous capacity for astonishing and untranslatable connections and constructions of the German language — all this Turkish music in Heidegger's philosophical language is the *correct* expression of his fundamental thesis that, through the usual language, we hide for ourselves the real structure of the world, that the habitual language expresses "das Verfallensein" ("Being-in-decay") with its absence of mind, and prefers "Vorhandensein" ("Being-at-hand") to "Zuhandensein" ("Being-by-hand") and "Dasein", which is just the contrary of the real state of affairs. If Heidegger had written an ordinary language, he would have abandoned his philosophy. He would have remained within the everyday existence, with its concealment of reality. Heidegger means that the ontological analysis is very hard to express, because what is ontically most close to us, is ontologically the most remote — since our everyday existence conceals reality. The ontical phenomena which are closest to us are just those which it is hardest to analyse ontologically. "Das ontisch Nächste und Bekannte ist das ontologisch Fernste, Unerkannte —" (*Sein und Zeit,* 43; 15). ("What is ontically closest and familiar, is from the ontological point of view that which is farthest away, the unknown.") The difficulty of the ontological analysis manifests itself in his difficult terminology. Its striking peculiarity has a special philosophical function: to make the ontically hidden ontologically clear.

To these philosophers language is not only an organ for communication of truths about the world. To philosophers with double awareness the language itself is not only a neutral means of communication but is dependent upon the author's "that"-consciousness and its character.

Wittgenstein says that the world of the happy and the world of the unhappy are different, and that different ethics do not involve a change *in* the world but *of* the world, that they do not change facts in the world but the limits of the world. For Wittgenstein it is not language which can give this picture of the world's "Faktizität". But language determines what goes for truth and what the essence of the things is. "*Essence* is expressed by grammar". (*Phil. Invest.*, 371; 373). The question of which language to use is therefore just the question of the world's "Faktizität", not its "Tatsächlichkeit" and it is answered when man's form of life becomes clear to me. In this both the earlier and the later Wittgenstein agree. "In brief, the world must thereby become quite another. It must so to speak wax or wane as a whole." (6.43). The difference between Heidegger and Wittgenstein here is that Heidegger uses the concept "truth" for this existential state, whereas Wittgenstein (*Tractatus*) means that truth is not a quality; for Wittgenstein only statements are true or false. Man's form of life determines what shall pass for truth, according to Wittgenstein, whereas Heidegger holds that the truth already lies in man's form of life. In Heidegger's words: the truth resides in the relationship "Sein" — "Dasein". (This relationship itself is given different interpretations by the earlier and the later Heidegger).

For Heidegger (*Sein und Zeit*, 194) the various phenomena of the will are derived from "Sorge" (cura), which he regards as a philosophical concept. It is implicit in the fundamental structure of Heidegger's philosophy that "Dasein" is always "sich vorweg". (*Sein und Zeit*, 236). Heidegger's philosophy is teleological philosophy. In Wittgenstein: my *choice* of form of life decides my *language*. This fact is not altered by the tendency in Heidegger's later philosophy to stress "Sein" and not "Dasein" (instead of dedicating himself to "Analytik des Daseins" ("Analysis of Being-there") he becomes a "Mysten des Seins". (Löwith: *Heidegger, Denker in dürftiger Zeit*, 21).) "Wissen" and "Wollen" cannot be separated. The truth is found in their unity. (*Holzwege*, 55). Only apparently is knowledge neutral. The active will is higher than knowledge and determines it. (Not the will as phenomena, not "der Wille" but "das Wollen" is at stake (6.423, 6.43). This is of course even more explicit in Heidegger). The will determines the wholeness of the knowledge, not its contents: "The world is independent of my will." (6.373). This is where the concept "das Höhere" in Wittgenstein (*Tractatus*) has its root. Therefore ethics, as belonging to "das Höhere", is transcendental; it cannot be expressed in sentences.

9.

The unity of "Wissen" and "Wollen" (Heidegger)[2], that the language games are determined by forms of life (Wittgenstein) becomes manifest in Heidegger's and Wittgenstein's view on the *question of values*.

The questions of values belong to the sphere of silence (Heidegger), of the mystical (Wittgenstein), in which "das Höhere", e.g., the ethics belongs. The meaning of the world lies outside itself. There are no values in the world, only "outside all happening and being-so". There are no ethical sentences. Ethics become transcendental (6.41; 6.42; 6.421). The bad or the good will does not modify any facts in the world, it changes the world, not the contents of the world (6.43). In our terms: the world's "that", not its "what" is the aim of ethics. The question of *qualities* of value perishes, since qualities exist only *in* the world. But this does not imply that the value-nihilists are right. On the contrary, they have not seen the realm within which the values exist (6.522). They have only simple consciousness, i.e. they are, in our sense, unconscious. If the question of qualities of values is raised on the level of matter-of-fact-consciousness the problem of values cannot be solved.

As the question of value concerns the relationship between the matter-of-fact-consciousness and the "that"-consciousness (in order that the question may be meaningful) the answer cannot be given as if it concerned relations only on the plane of the matter-of-fact-consciousness.

10.

G. H. von Wright says (in "Dagens Nyheter" 16.3.1957: "Wittgenstein's 'Tractatus'") that Wittgenstein's views on the questions of value are similar to those of *Hägerström*. According to what has been said above, their concepts of the philosophical status of values are qualitatively different. Their difference discloses itself also in their diverging views on *metaphysics* in general. The same superficial resemblance with the value-nihilists is found in Heidegger. He criticises the objectivism of values, which regards values as particular quantities having a special form of being. (*Platons Lehre von der Wahrheit*, 99 ff). On the contrary, Heidegger incorporates the questions of values into his philosophy as a whole. They cannot be separated from it. It is the "wertbehafteten" things ("things, to which we attach value") that really show in which world we live. The "wertbehafteten" things are "das Zu-handene", ("being-by-hand"), and have priority in relation to "das

Vorhandene" ("being-at-hand"). (*Sein und Zeit*, 63—64). Hägerström is the great enemy of metaphysics; he tries to destroy it. Metaphysical statements are meaningless. Wittgenstein can apparently say the same. But — he declares his own philosophy to be metaphysics. For him metaphysics is statements about the inexpressible. Such statements are, according to Wittgenstein himself, meaningless as statements (belonging to the world of language). But in spite of this, he utters them, and wants to utter them. Certainly, he is very ascetic in his metaphysics, saying as little as possible about that which cannot be said. But this does not change the fact that he is doing metaphysics, that he declares that "das Höhere" exists, the mystical, the inexpressible. There is an essential difference between Wittgenstein and usual metaphysicians. Wittgenstein does not, like other metaphysicians, try to rehabilitate his metaphysical statements on the level of the world of language. Wittgenstein is rigorous in setting boundaries between the world of language and the mystical. Thus his *double concept of truth:* he must consider his metaphysics, i.e., his statements about the mystical, as meaningless (in the sense of the intellectual say-truth). Nevertheless he declares his philosophy to be absolutely true (in the meaning of the "existential" show-truth). The double awareness leads to a double concept of truth. Wittgenstein does not mean as Hägerström that the values are meaningless hypotheses. They exist. What Wittgenstein says is that they don't exist in the world. Ethics cannot be expressed in sentences; but he nevertheless talks about ethics as "das Höhere". Everything is "joint in an endless natural connection, where nothing in itself is lower or higher." (Hägerström). But — "das Höhere" exists! (Wittgenstein).

Heidegger is different. He rejects the traditional occidental metaphysics from Plato to Nietzsche. This is not done out of an antimetaphysical tendency, but because the traditional metaphysics belongs to the level of "das Seiende". Thus it conceals "Sein", "Sein" which for Heidegger is, beyond all "Seiendes", the real being. (*Platons Lehre von der Wahrheit*, 53, 80; *Was heisst Denken?*, 35).

Without this double concept of truth there would have been an evident contradiction between what Wittgenstein says in 6.53, 6.54 and what he says in his preface: "On the other hand the truth of the thoughts communicated here seems to me unassailable and definitive". The preface must be concerned with the truth of what Wittgenstein *shows* in the exposition itself in *Tractatus*. He denies the truth of what he *says*. What Wittgenstein shows is true, but as he shows what he aims at by means of philosophical statements, he must reject what these

statements show, because that which can be shown, cannot be said. "What *can* be shown *cannot* be said" (4.1212).

Wittgenstein is conscious not only of how the world is, but also of what he calls the mystical, what cannot be expressed; *that* it is. (6.44). It may be compared with Heidegger's: "Das im Verstehen als Existenzial Gekonnte is kein Was, sondern das Sein als Existieren." (*Sein und Zeit,* 143). All knowledge, "Anschauung", "Denken" and phenomenological "Wesensschau", "gründet im existenzialen Verstehen." (*Sein und Zeit,* 147). ("The existentialia which you are able to be in understanding is not a what, but the Being as existence. All knowledge, "experience", "thinking", and phenomenological "Wesensschau" is rooted in existentialistic understanding"). I find that the difference between Heidegger and Wittgenstein is not a difference of kind, but of degree; Wittgenstein is extremely ascetic in his metaphysics, in saying (showing) with words what cannot be said, the mystical. Heidegger cannot be charged with such a metaphysical asceticism; he does not attempt to reduce the number of metaphysical statements to a minimum.

11.

Dread expresses our attitude towards the world's "Jemeinigkeit", ("nature of being ever mine"). Dread does not relate to any object, because objects exist only within this world. Its "Jemeinigkeit" is not a quality *in* the world. It is a nothing. It is the object of the dread: nothing. This is Heidegger's solipsism.

In "Dasein", "Sein" is for Heidegger always "Jemeinigkeit". (*Sein und Zeit,* 42). In dread this *existential solipsism* is revealed. What is at stake is not any kind of categorical solipsism; only in the "Dasein" as conceived ontologically do we live in the loneliness of dread. This is so because the world is not something in which I find myself, but something which is always strange to me. (*Sein und Zeit,* 188).

The world is always my world, according to Wittgenstein. And: "I am my world". (5.63). But the metaphysical I does not belong to the world. In Heidegger: the "Jemeinigkeit" of "Dasein" (and thus that of the world, because "Dasein" is characterized by being "In-der-Welt-sein") is not a quality of the world. Likewise, for Wittgenstein the fact that I am my world is not a quality of the world. The solipsism thus becomes the basic philosophical position, the position from which the philosophical activity is carried on. This is, however, something which cannot be said, it shows itself (5.62).

On the view of Heidegger and Wittgenstein, the question of the existence of the outer world becomes meaningless. Solipsism usually implies that *the external world* is denied. Not so with Heidegger's and Wittgenstein's solipsism. For Heidegger: "Dasein" is always "In-der-Welt-sein" (*Sein und Zeit*, 202). For Wittgenstein: The world is always my world. (5.62; 5.63). The meaninglessness of the question is due to the fact that the two levels of consciousness meet here: "das Vorhandene" and "das Dasein", (Heidegger), say and show (Wittgenstein). If the existence of the external world is denied, this implies that "das Dasein" should not be "In-der-Welt-sein", (*Sein und Zeit*, 206), but "Dasein" should be something isolated from the world. It would be to regard "Dasein" as "vorhanden". If the existence of the external world is asserted, this implies that I am unaware that the assertion of the external world cannot take place in the world, but only in "das Dasein". This should imply that "das Vorhandene" would be put on a level with "das Dasein" (Heidegger), the world equated with the metaphysical Ego (Wittgenstein). "Das Dasein" should be transformed into "das Vorhandene" (Heidegger). "Der Grund dafür liegt im Verfallen des Daseins und der darin motivierten Verlegung des primären Seinsverständnisses auf das Sein als Vorhandenheit." (*Sein und Zeit*, 206). ("The reason for this is to be found in the decay of the Being-there and the shifting of the primary understanding of Being into the Being as Being-at-hand, which is motivated by it.") The inexpressible should be something *in* the world, should belong to that which can be said (Wittgenstein).

13.

This same circumstance makes *the absolute doubt* impossible (*Sein und Zeit*, 226—229). To believe in the possibility of the absolute doubt is not to have the double awareness. It is to let the matter-of-fact-consciousness answer a question which concerns the relationship between the matter-of-fact-consciousness and the "that"-consciousness. It is to be unaware of the existential presuppositions of the thought. What thinks, is always the metaphysical Ego (Wittgenstein). The thinker is always bound to his "Dasein" (Heidegger).

"Then what we do in our language-game always rests on a tacit presupposition." (*Phil. Invest.*, 179e). Even *all* doubt is based on such existential presuppositions. Hence, the absolute doubt is an impossibility.

The doubt is possible only as a language-game, and all language-games are determined by forms of life. "What has to be accepted, the given, is — so one could say — *forms of life*." (*Phil. Invest.*, 226e). The doubt presupposes itself a language-game and is thus already existentially determined by a form of life. "Doubting has an end". (*Phil. Invest.*, 180e). There the doubt *must* end (N. Malcolm: "Wittgenstein's Philosophical Investigations". *The Philosophical Review* 1954, 547).

According to the *Tractatus:* one can only doubt *within* the limits of the thinkable and the expressible. Doubt beyond those limits is a meaninglessness; what is said can be doubted, but not that which is shown. "Scepticism is *not* irrefutable, but palpably senseless, if it would doubt where a question cannot be asked." (6.51).

According to Heidegger: Of all truth — even the truth of the doubt — it holds that it ". . . *ist . . . relativ auf das Sein des Daseins.*" (*Sein und Zeit,* 227). ("". . . is . . . related to the Being as the Being-there""). "Dasein" is always "in the truth" (*Sein und Zeit,* 226), since the truth is a determination of Sein. Therefore the absolute doubt is an impossibility. The *real* sceptic need not be refuted; he has already done it himself, because he has "in der Verzweiflung des Selbstmords das Dasein und damit die Wahrheit ausgelöscht." (*Sein und Zeit,* 229). ("In the despair of the suicide he has extinguished the Being-there and the truth with it.").

Heidegger's and Wittgenstein's philosophy is existential presupposition-thinking; the thinking proceeds from something existentially given, from the concrete "Dasein" of the thinker (Heidegger), from the form of life of the thinker (*Phil. Invest.*). The philosophizing Ego is the metaphysical subject, which stands within the realm of the unthinkable and the inexpressible and from there sets the boundaries of the thinkable and expressible. (*Tractatus*). To ask sceptically for the basis of this, is, even for Heidegger, to ask a meaningless question, to which no answer is possible. "Dies Unmögliche liegt nicht daran, dass unser menschliches Denken zu solchem Erklären und Begründen unfähig ist. Vielmehr beruht das Unerklärbare und Unbegründbare des Weltens von Welt darin, dass so etwas wie Ursachen und Gründe dem Welten von Welt ungemäss bleiben." (*Vorträge und Aufsätze,* 178). ("That this is impossible does not lie in the fact that our human thinking is unsuited to give such explanation and reasons. Rather, the inexplicable of the world and the fact that no reason can be given for the world, e.g. that it is, is caused by the fact that such things as causes and reasons remain inappropriate to the world: that it is.").

The *ontological* analysis by Heidegger is necessarily built upon the *existentialistic*. This Heidegger founds in the relationship "Sein"— "Dasein". For "Dasein" is in itself as "Dasein" ontological, is already (although concealed) related to "Sein". (*Sein und Zeit,* 13). Our thinking necessarily takes place here in this relationship. "*Kein* Weg des Denkens, auch nicht der des metaphysischen, geht vom Menschenwesen aus und von da zum Sein über oder umgekehrt vom Sein aus und dann zum Menschen zurück. Vielmehr *geht* jeder Weg des Denkens immer schon *innerhalb* des ganzen Verhältnisses von Sein und Menschenwesen, sonst ist es kein Denken." (*Was heisst Denken?,* 74; 28, 45; *Holzwege,* 81; *Einführung in die Metaphysik,* 106 ff., 110, 134.) ("No way of thinking — not even the metaphysical one — originates from human essence and therefrom passes to Being or, *vice versa,* originates from Being and passes back to man. Rather, every way of thinking *goes* always already *inside* the entire relation between Being and human essence, otherwise there is no thinking.").

But this *existentialistic analysis* of "Dasein", which is the condition of the ontological analysis of "Sein" and is not separable from it, is in its turn founded in *existential experience.* In the existential situation lies already preformed, in embryo, the existentialistic insight (which man, true enough, always flees). Not only is this ontological "Sein" combined with the existential "Dasein". But this "Dasein" is always given as ontic existential experience and has already as such "Vorverständnis" of the existentialistic-ontological "Sein". According to Heidegger's own philosophy the ontological-existentialistic analysis is necessarily founded in an ontic-existential position. The existential position, based on an experience which awakes the existentialistic-ontological insight, is very rare. It is not rare as experience, but only seldom can it be resolved into existentialistic-ontological insight (Heidegger feels alone among his contemporaries on this point).

Only from this ontical-existential experience, one of many experiences, comes the ontological-existential analysis into being. From this special experience the general analysis of "Dasein" can proceed. With this special experience as material the whole essence of "Dasein" is revealed (and because "Dasein" is in itself "Seinsverständnis", the essence of being is revealed with the essence of "Dasein"). Heidegger is therefore conscious of the fact that *all* truth, all knowledge, is founded and rooted in a particular *existential* position. Even Heidegger's own exist-

entialistic analysis is thus existentially founded. Knowledge and thinking are parts of our general attitude towards life; they depend upon and express an attitude of life.

What corresponds to this in Wittgenstein is what he calls "*forms of life*" (*Phil. Invest.*, 226e).

Man's form of life determines man's language-game. Man's way of living and thinking decides what he lets pass as truth. "One could hardly place too much stress on the importance of this latter notion (form of life) in Wittgenstein's thought. It is intimately related to the notion "language-game". His choice of the latter term is meant to bring into prominence the fact that the *speaking* of language is part of an activity, or of a form of life." (23; of. 19). (N. Malcolm: "Wittgenstein's Philosophical Investigations". *The Philosophical Review* 1954, p. 550). This may be called Wittgenstein's existentialism.

But the implication and meaning of the ontological-existentialistic interpretation is by no means given through the dependence of Heidegger's ontical-existential position. According to Heidegger, his ontological interpretation is generally valid and universally applicable. His claim is hence at least as big as, for example, Hegel's. He will not present his ontical-existential position but an ontological-existentialistic interpretation. He will not present a view of the world or of life but an interpretation that comes "before" all views of life and the world and makes them possible.

But Heidegger is not unconscious of the fact that his ontological-existentialistic interpretation (whose Heideggerian originality he is well aware of) is founded in his own particular ontical-existential position. This ontological-existentialistic interpretation is given from an ontical-existential point of view. He has seen that all interpretation, in order to get started, must be driven by something in the individual philosopher (Heidegger), who does the interpretation. An ontological interpretation does not make itself, but is done by somebody, who must have an existential occasion for doing it and making it a part of his own history.

Expressed in Heidegger's own way: the truth is "ontisch nur im 'Subjekt' möglich —" (*Sein und Zeit,* 227). ("the truth is ontically possible only in the 'Subject'").

Heidegger himself recognizes that his ontical-existential position is the cause of his ontology. This is where this article ties on to Heidegger himself. He has himself seen the circumstances which we try to clarify here. But by exposing this standpoint and the experience on which it is

founded, no total view of Heidegger's philosophy is given. On the contrary, an exposition of this kind presupposes that Heidegger's ontology is not contemplated separately. It is impossible to consider it an important task to see the experience behind Heidegger's philosophy, and at the same time to move philosophically within this ontology. If the ontology with its universal claim is accepted, the determination of Heidegger's ontical-existential position should be a task of no value. Heidegger himself mentions it only to point out that he will *not* expose it. The ontical-existential position-experience is for Heidegger himself only the catalysator which starts the philosophical process. It does not itself participate in the philosophical process. Heidegger is not an existentialist in the ordinary sense except by being aware of the catalytic character of the existential position. (His existentialism has nothing directly to do with what Heidegger calls existence. See e.g. *Platons Lehre von der Wahrheit,* 77 ff.).

The later Wittgenstein is, as we use the word here, an existentialist in a more unlimited way. To him the existential position (forms of life) are not only catalysts. By deciding which language-games are played, the existential positions determine in an unlimited way what shall be taken for truth.

Heidegger does not make the ontical-existential position a central object of investigation. He does not consider "Sein", but something "Seiendes". In his own words, this is a form of "Verfallensein" ("Being-in-decay"). He will not give a new interpretation of something (not even of all) "Seiendes" but of "Sein". He is not at all interested in the "Seiende" upon which his own ontology builds. But *this* ontical-existential position of Heidegger is what this article seeks to attain by means of Heidegger's work with its ontological-existentialistic intentions. Different characteristics and concepts in Heidegger's philosophy must be guides to the central ontical-existential position, which "underlies" the centre of Heidegger's ontology. (We believe that we have seen this centre as far as it is relevant to our task.) *Only* that part of Heidegger's philosophy which is necessary to show this has been utilized.

In order to make Heidegger's ontical-existential position a central problem, as is done in this article, the framework of the discussion must not be Heidegger's ontology. That is, in this article, we do not accept Heidegger's claim that his ontology is universally valid. *Why* it is not accepted needs no explanation, only *that* it is not accepted. Only then is the task possible. That Heidegger himself — as already pointed out — has seen the relationship between his ontical-existential position and his

ontology, does not imply that he means that it is possible to let this relationship be a central problem *within* his ontology. On the contrary, to do this is, according to Heidegger, a phenomenon of "Verfallensein",

An exposition of Heidegger's philosophy, so organized that the relationship ontical-existential and ontological-existentialistic was studied as a whole, could perhaps have been a successful undertaking. Only Heidegger's ontical-existential position will be explained here, not the problem of its relationship to the ontology as a whole, though the explanation necessarily comes from the ontological-existentialistic interpretation Heidegger has given his ontical-existential experience.

A total view of Heidegger's philosophy is perhaps implicit in this undertaking (but this total view of Heidegger's philosophy cannot be similar to Heidegger's own). It is, however, not necessary to acknowledge as true this implicit total view on Heidegger in order to recognize as correct this exposition of Heidegger's ontical-existential position. Even an incorrect view of Heidegger's philosophy as a whole may perhaps give suggestions and hints about it.

Already the exposition of Heidegger's ontical-existential position goes counter to his ontology. Moreover, even the comparison between Heidegger and Wittgenstein is impossible within Heidegger's ontological frame. For Wittgenstein cannot be interpreted as presenting an ontology in Heidegger's sense. And only in that case would a comparison within Heidegger's framework be possible. Nor can one exploit Wittgenstein as Heidegger, within his ontological interpretation, has exploited philosophers whose ontology he regards as false; for example, Plato and Nietzsche (this is possible only for Heidegger, not for one who stands outside Heidegger). "That"-experience and -consciousness does not "underlie" Wittgenstein's philosophy, as it underlies that of Heidegger. It is not a part of Wittgenstein's philosophy, although it is expressed in his philosophical writings and is, in my opinion, the prime mover of his philosophy. The "that"-consciousness is not the centre of what he says but the expression of that which, according to Wittgenstein, cannot be expressed: the mystical, (*Tractatus*). But that which lies beyond the expressible is the important. Wittgenstein's "that"-consciousness is not a part of his philosophy in the same way as in Heidegger. The consequence of this is that it is not necessary to evaluate Wittgenstein's philosophy (in the case of Heidegger, one had to reject the universal claim of his philosophy in order to make possible the present task.). Towards Heidegger's philosophy a (negative) position must be taken; within Wittgenstein's philosophy no evaluation is nec-

essary here, be it positive or negative (because the "that"-consciousness is not a part of his philosophy, but remains beyond its limit.).

A comparison between two philosophers should usually fulfil one requirement: that it starts from the centre of these philosophers, so that the comparison does not become a comparison between disparate utterances, a comparison which is of no importance. This natural claim must here be rejected as impossible. A Heidegger—Wittgenstein comparison is rewarding, but not from their philosophical centre. The standard of comparison in Heidegger is not important to Heidegger. On the contrary, such a comparison as this shows that there has been no attention to that which is central for Heidegger. (On the other hand, as mentioned, he does not deny that even for him the ontological-existentialistic analysis depends on the ontical-existential experience.) The standard of comparison by Wittgenstein is not philosophically central to Wittgenstein; this is a consequence of the character of Wittgenstein's philosophy both in its earlier and in its later form. For Wittgenstein's philosophy is not a doctrinal system but Wittgenstein's philosophical activity. Therefore it is misleading to talk about a centre of his philosophy. On the other hand it is meaningful to ask for the motive power in his philosophical activity, and this article has tried to give *one* answer to this. Heidegger elevates himself above his existential "that"-consciousness to the ontological plane of philosophy. Because of this, he does not pay attention to the relationship between ontical-existential and ontological-existentialistic which he has seen himself. Because Heidegger regards his ontological analysis as simply universal for all ontical-existential experience, his own ontical-existential position cannot be of any interest. Wittgenstein, on the contrary, descends from his existential "that"-consciousness to the plane of philosophy in order to free philosophy from all claims to state something factually important (and in this negative way express the important realm of the inexpressible). Therefore Wittgenstein in his philosophy is not able to express his "that"-consciousness.[3]

NOTES

1 Because of this Heidegger and Wittgenstein are quite different in the use of the concept "Sinn": "Only the proposition has sense; –" (3.3), whereas Heidegger holds that "Sinn ist ein Existenzial des Daseins, nicht eine Eigenschaft, die am Seienden haftet, – *Nur Dasein kan daher sinnvoll oder sinnlos sein.* (*Sein und Zeit*, 151). ("Sense is an existentialistic function of Being-there, not a quality attached to the What-is, – Only Being-there can thus make sense or be senseless.").

2 It is certainly important to remember that Heidegger studied medieval philosophy, and especially the "voluntarist" Duns Scotus, carefully. Heidegger's work about the latter is helpful for the understanding of Heidegger's position, as Löwith has rightly pointed out in *Heidegger, Denker in dürftiger Zeit*.

3 No distinction is here made between the earlier and the later Heidegger and Wittgenstein. The distinction is not denied, but it does not concern our task, which is to study the double awareness in both philosophers and see its origin in a "that"-experience. We don't ask whether and why the "that"-consciousness is purer or stronger in the earlier or the later Heidegger, or in the earlier or the later Wittgenstein. Sometimes a thought in the *Tractatus* is shown to have a parallel in the *Phil. Invest.* and *vice versa.* (This is done in order to bring into prominence that Wittgenstein's philosophy in all its stages had an existential foundation.)

WHAT SHOULD WE SAY?

Preliminary summaries of a few empirio-semantical investigations[1] concerning such sentences as: *can we say x, should we ever (ordinarily) say x, x is self-evident* (tautological, contradictory, nonsensical), *P does not know what he is talking about, x is voluntary* (involuntary) and: *that is no excuse.*

by

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I. Hermeneutical Possibilism

"With the word the thought is born on man's lips; what is dimly said is dimly thought," said *Esaias Tegnér.*[2] "But words are imposed according to the understanding of the crowd," said *Francis Bacon.*[3] "And man has the capacity of speaking," said *Juan in America* (Linklater), "as monkeys have of swinging themselves by their tails." "Ich kann das *Wort* so hoch unmöglich schätzen," said *Faust.* And, after all, *Juliet* said: "What's in a name? That which we call a rose | By any other name would smell as sweet." "The question is," said *Alice* (in Wonderland), "whether you *can* make words mean so many different things." "The question is," said *Humpty Dumpty, "which is to be the master — that's all."*

The above contamination of familiar apophthegms seems to evince an attitude to the use of language for communication which may also be indicated by the following three characteristics:

a) A *general skepticism towards language* as a tool for thought, for interchange of ideas or, if one wants, as an instrument for transmitting messages (e.g. statements) between or within persons.

b) A marked *sensitivity to language ambiguities* and awareness of other causes of ineffective (inadequate, unobjective) inter- and intrapersonal communication.

c) An angelic *tolerance to* seemingly or apparently *inapt* sentences, expressions and other *verbal transmitters*[4] — combined with a *genuine willingness, readiness and ability to advance tenable hypotheses as to what message — if any — a known or imaginary communicator may*

have attempted to transmit by means of a given locution, its ostensible formal defectiveness notwithstanding.

It is both the underlying assumption and the moral of the empirio-semantical enquiries mentioned in this article that some sort of Humpty-Dumpty sovereignty is an important prerequisite for an effective (objective) inter- and intra-personal communication. The effective communicator will develop a freely manipulating attitude towards the language. He will certainly be relieved from any form of linguistic taboos and commandments ("Sin against the word," "sin against language" etc.), from any kind of linguistic rigidities or anything that, in general, might induce in him a doubt that the store of available linguistic facilities and means of verbal expression is not at all times absolutely unlimited. His battle cry rings out: "*Anything is possible!*" That is: "Any transmitter (*e.g.* any sentence) may transmit any message (*e.g.* any statement)." Consequently he must also militate against any type of absolutism, clearcut dichotomies,[5] necessary laws, unshakable data (perceptions, experiences . . .) and incorrigible sentences. Nothing controls what he *can* (is able to) say and mean. His problem is: "*What should we say?*" and his answer: "Anything that under the given circumstances seems to transmit objectively that which we sincerely believe we want to express."

II. Linguistic Rigidity

An apt idea as to what one frequently means by "linguistic rigidity" is given in the story about a scientist doing research on primitive Indian dialects, who was trying to find out how one particular tribe transmit in their dialect a message for which a customary English transmitter would be the sentence: "I am ill." This was carefully explained to the informant, a tribe member who willingly and even pleasantly said in his native tongue: "You are ill," "he is ill," and so forth. But when the scientist persists, trying to get him to utter the desired sentence, he says, his face darkening with anger: "No! I am not ill!" The reason why the Indian felt reluctant to utter the sentence "I am ill" is often sought in his alleged inability to discriminate between a) language symbols (transmitters) and b) what language symbols supposedly convey (the message) or "stand for" (the objects).

It is clear that, in order to select the most appropriate transmitter for his message the effective communicator must not be hampered in his free choice by linguistic rigidity. But is there any danger? According

to such prominent psychologists as Jean *Piaget* and L. S. *Vigotsky,* there is. At least they both agree that we all as young children have been inclined to "perceive a word as an inalienable *attribute* to the object referred to"[6] just as we did the colour of the object.

Vigotsky claims to have shown that behind the words there is an independent grammar of thought, what he calls "the grammar of word-meanings."

"Since the semantic and the vocal aspects of speech do not coincide, verbal expressions cannot come into existence in their final form, but have to develop gradually. To achieve the division of speech into semantics and phonetics the child has to differentiate between the two aspects of speech and understand the nature of this difference. At first the child uses verbal forms and verbal meanings without being conscious of them as such and without differentiating them. *A word is for the child a part of an object or its quality, which cannot be separated from its other qualities. Such a conception, it seems, is common to every primitive linguistic consciousness.*"

"Simple experiments with children show that in the pre-school age, children explain the names of objects by their qualities. An animal is called "cow" because it has horns; "calf" — because its horns are still small; "horse" — because it has no horns; "dog" — because it is small and has no horns; an object is called "motor-car" — because it is not an animal at all. When children are asked whether it is possible to replace the name of one object with that of another, for instance to call a cow — "ink," and ink "a cow," they answer that it is entirely impossible, because ink is used for writing and the cow gives milk. The exchange of a name means for them also the exchange of the qualities of the objects, so close and inseparable is the connection between the two. In one experiment the children were instructed to change the names of some objects, for instance to call a dog — cow, and then had to answer questions about these objects. "Has a cow horns?" "Yes, it has." "But the "cow" is really a dog, and has a dog horns?" "Of course, if a dog is a cow, if it is called cow, then there must be horns. Such a dog which is called cow must have little horns." *This example shows how difficult it is for a child to separate the name of an object from its qualities and how these qualities cling to the name when it is transferred — just as possessions cling to their owner.*"[7]

This excerpt is interesting for various reasons. If tenable, it might for one thing explain why some philosophers try to solve such problems as whether we will ever be able to chew milk or admire a landscape

with closed eyes, by answering questions such as "*Can* we (ever) say: "He admired the landscape with closed eyes"?" "Can we (ever) say: "She was chewing the milk"?": it manifests a regression to the pre-school age!

However, simple experiments with children[1,7] show that in the pre-school age children have no difficulties whatsoever in calling a dog "a cow" a basketball "a bicycle" or a button "an ocean" — *nota bene: as soon as they grasp what the playground instructor wants to communicate to them.* The only obstacles to overcome are certain ambiguities in the expressions "*can*", as in, e.g., "can we say (— call)?", and "possible", as in "is it possible to say (— call)?" If we call these sentences T_0, the two most important directions of precization may aptly be indicated by T_1 and T_2 below.

T_1 is it (technically, etc.) within our power to utter (write) . . .

T_2 is it permissible (correct, in place, not silly, etc.) to utter (write) . . .

T_0, however, when interpreted in the direction of T_1 becomes so obviously true that no one would ever dream of expressing such a triviality.[8] Naturally, we are all capable of pointing to a basketball and uttering: "That is a bicycle." Consequently, under these circumstances, T_2 is bound to appear to the pre-school children (and to some extent to all the other age-groups tested) as *the only plausible* of the two directions of precization. However, no sooner was the T_1 direction of precization indicated to them than all their hesitations in calling a dog "a cow" disappeared.

The procedure employed was quite simple. The 120 pre-school children were asked six questions of which the last three were the same for all. They read as follows:

4. "Can you call a dog a cow?"
5. "Can you call a basketball a bicycle?"
6. "Can you call a button an ocean?"

But of the first set of questions we had two types: one, the "potentiality questionnaire", which was supposed to bias the respondent in the direction of a T_1 type of interpretations of "can," and another, the "*permissibility* questionnaire," constructed to support a tendency to a T_2 type of interpretations of "can." The potentiality (T_1) questionnaire started out with the following three questions:

1. "Can you swim in salt water?"
2. "Can you swim in milk?"
3. "Can you say: 'antidisestablishmentarianism?'"
   (or "can you spell "eucalyptus"?")

The corresponding questions in the *permissibility* (T_2) questionnaire were:

1. "Can you judge a man by his appearance?"

2. "Can you play hookey?" (or: "come to class with no clothes on?" or: "swear in class?")

3. "Can you call teachers by their first name?" (or "call a policeman 'a stupid flatfoot' to his face!")

All the 60 children who were asked the permissibility (T_2) questions (1, 2 & 3) answered questions 4, 5, and 6 in the negative, i.e. as one would have expected according to Vigotsky (Piaget, Frazer and others). However, their reasons for this attitude were by and large not concerned with principle impossibilities, but with *practical difficulties* in adopting this "silly" way of speaking: "The other kids wouldn't know what I was talking about..."

On the other hand: only 16 of the 60 who were asked the potentiality (T_1) questions (1, 2 & 3) answered "*no*" to one or more of the last three questions. The other 44 offered a well-founded "yes" even to question number 6. And although some of them agreed that it was a silly thing to do, they did not for a moment seem to question their own capability of calling a dog "a cow," a basketball "a bicycle" or a button "an ocean."

Then we started the "word game" with the entire sample of preschool children. Every one was told "the story about a boy, Johnny, who played a funny word game to entertain his friends. He called things by names which people usually used for other things. For instance, he called his dog's ball "a fish," and he called the dog "a cow." His friends would laugh when Johnny said, "Look, my cow is bringing me his fish!".[9]

"When you answer the rest of the questions, try to play a word game where you use the word "cow" for *dog*, and the word "dog" for *cow*. Everything that used to be true for cows will be true for *dogs*, when you play this word game. Everything that used to be true for dogs will now be true for *cows*. For instance, when you play this word game, it is now *true* that cows can bark, and it is now *false* that dogs can bark. Please try to play this word game in the rest of these questions."

The rest of the questions were: "Can *dogs* moo (low)?", "Can *dogs* bark?", "Can *dogs* have horns?", "Can *cows* have horns?", "Can *dogs* give milk?", "Can *cows* have calves?", "Can *cows* have puppies?", "Can *dogs* have collars and bury bones?", "Approximately how many

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cows are there in Berkeley?" The children were also asked to state the *reasons* for their answers.

Not a single one of the 120 pre-school children was incapable of playing with words in the indicated manner. A few of the more absent-minded or easily distracted young scatterbrains seemed to lose the thread every now and then, but straightened themselves out again, when it came to giving the reasons for their answers. They all enjoyed playing the game — so they said — and continued playing it on the playground as soon as they had received the instructor's permission to do so.

These experiments were repeated — *mutatis mutandis* —: in the primary school with 4—6 graders,[1,8] in the junior and senior high school, on college level[1,10] and with older people.[1,9] We found the same pattern everywhere: When given the impression that by the formulations, T_0: "can you call (— say) . . ." and "is it possible to call (— say) . . ." and "do we ever call (— say) . . ." we attempted to transmit to them something that might have been somewhat less ambiguously conveyed by T_2: "is it permissible to call (— say) . . .," these respondents all reacted amazingly like the pre-school children in Vigotsky's experiments. Whereas respondents who were brought to believe that T_1: "is it within your power to call (— say)" was more what we wanted to express by T_0, showed absolutely no symptoms of inability "to distinguish designations and designata" as far as this is measured by negative answers to such questions as "can you call a dog a cow?"

All the respondents — except the pre-school — were also confronted with a time — consuming questionnaire constructed in cooperation with the late Professor Else Frenkel-Brunswik to measure '*linguistic rigidity*.' But no significant correlations were found of any relevance to problems dealt with in the present article.

On the other hand, the fact should not be left unmentioned that *26⅔ per cent of the pre-school children of the "potentiality"-group d i d nevertheless answer "n o" to one or more of the three last questions in the first part of the enquiry.* This is nearly four times the percentage of "no"-answers in any other corresponding group of our sample (6⅔ per cent in a socio-economically "lower-lower"-grammar school). Thus, one may feel tempted to wonder whether there may be a speck of truth in Vigotsky's theory after all? ("For the pre-school child the name is [more of] an inseparable part of the object?") Frankly, this is not very likely. Other explanations of the 16 pre-school deviants seem far more plausible. Not only is the "permissibility"

direction of precization much more predominant in minor children ("Can I go out?", "Can I have a candy?", etc.), but in addition to that they are still at the stage where already the art of *correct usage* of words in itself is a brand new and challenging game. Consequently they are anxious to demonstrate their proficiency. Their reaction is the one of a novice in a difficult sport: they are solely concerned with their "doing it *right*." — To support this notion we would need respondents from an older age group, but with the same overwhelming desire to evince their increasing skill in a correct usage of words. We found to excess what we wanted in the California State School for the Deaf.[1,11]

For the group of deaf students who had been taught the "potentiality" (T_1-) interpretation[10] of "can," these were the results of the first part of the test:

Age	12	13	14	15	16	17	18	19	20	21	22	Sum
"No" to can you "call. . . . . ."			2	1	10	6	7	7	5		2	39
"Yes" to "can you call. . . . . ."	1		1	2	4							8
												47

Even more amazing were the results from the second, the word-game-playing part (where all the pre-school children, as we have seen, did so splendidly):

Age	12	13	14	15	16	17	18	19	20	21	22	Sum
Unable to play the word game				7	22	7	1	5	5		1	48
Able to play the word game	1		4	7	11	10	10	8	2			53
												101

Had the sample been on an average ten years younger, the above results would probably be cited in support of Vigotsky's theory, although some of the reasons offered by the deaf students would have puzzled and disappointed him ("I haven't been told that I can [may] play this game" etc.). And with the present age-group — up to twenty-two — it is questionable whether Vigotsky has not found any traits of the so-called "primitive" people's alleged *linguistic rigidity* in his pre-school children. The results are most convincingly explained as being due to an ineffective verbal communication between Vigotsky and the children, caused by certain ambiguities in such linguistic expressions as "can"

and "is it possible to." Moreover, this lowering of communicability may plausibly stem from Vigotsky's failure to observe particularly the first two requirements for hermeneutical possibilism. He revealed a groundless semantical optimism, and what one tentatively might call "hermeneutical obtuseness" in so far as he naïvely assumed that he had succeeded in conveying to the pre-school children — or even to himself, for that matter — what he presumably had in mind as a dim notion, when he uttered: "Is it possible to call a dog a cow?" Hence he was effectively prevented from understanding that the children, when responding to his sentences, were answering questions altogether *different* from the questions he apparently believed he had asked. In conclusion we may say: Vigotsky, among others, *tried* to support the hypothesis that the so-called "primitive man's" (alleged) *rigid attitude* to the use of language symbols repeats itself inescapably in the early postfoetal ontogenesis of the "man of culture." What Vigotsky actually *did* support, however, was the hypothesis that there is an important notion of 'linguistic rigidity,' *e.g.* as opposed to 'hermeneutical possibilism,' in which linguistic rigidity might manifest itself almost anywhere, and not least among philosophers and scientists (including psychologists) — as unintentionally, but so convincingly demonstrated by Vigotsky himself in his "Thought and Speech." Three vague, overlapping and strongly interdependent specifications of 'linguistic rigidity', in the above suggested sense of "linguistic rigidity," are indicated in the ensuing sections by such expressions as: a) "logical clairvoyance" b) "hermeneutical obtuseness" and c) "intolerance of locutional curiosities."[11]

III. Logical clairvoyance and intolerance of locutional curiosities

In an article "On the Method of Revelation in Logical Analyses" (in Tennessen and Gullvåg: *Logical Analysis and Definiteness of Intention*)[12] the present author takes the field against "anybody who is convinced that it is easy to "*see*" what a word or sentence "*means*" in a given case, that it is easy to "*see*" whether a term is used in harmony with a given definition or not . . .," and, I might have added, I suspect anybody who persuades himself to believe that he can "*see*" whether a given *sentence* transmits a statement (proposition) which is bound to be, *e.g. normative* and not *descriptive*, or *analytic* and not *synthetic, logically odd,* not *logically orthodox, meaningful,* not *meaning-*

less and so forth . . . In contradistinction to such optimistic clairvoyance the possibilist attitude may be expressed as follows (using the somewhat deceptive statement/sentence model of thought): In principle, almost any sentence may transmit almost any statement. Or in other words, it is hard to conceive of two so *different sentences* that it would not also be possible to imagine some circumstances under which they were transmitting the *same statement* (for at least one communicator in at least one reference of communication in at least one language society). And vice versa, for seemingly *similar sentences* expressing *different statements*. Consequently, it seems safe to assume that given a formulation, T_0, and a list of plausible (but cognitively rather similar) precizations T_1, T_2-T_n, it would always be possible to phrase the precizations in such a way that the list at the same time furnished examples of every conceivable and relevant grammatical and syntactical category.

Consider for instance the following list of plausible and cognitively different transmitter alternatives (not all precizations) of T_0:

T_0 It is good to have a barrel of potatoes.

T_1 The advantages of having a barrel of potatoes in the house outweigh the disadvantages.

T_2 I wonder: should not one get oneself a barrel of potatoes?

T_3 Get yourself a barrel of potatoes!

T_4 One ought to have a barrel of potatoes in the house.

T_5 Oh! If only one had a barrel of potatoes! A *barrel* of potatoes! A barrel of *potatoes*!! My kingdom for a barrel of potatoes!!!

T_6 Does it not give a delightful feeling to have a whole barrel of potatoes in the house(!)(?).

An adroit philologist would probably make nothing of picking out from this short list of transmitter alternatives of T_0, examples of a good many grammatical categories concerning sentences; such as: a declarative sentence, T_1 (T_4 and T_6?) (b) interrogative sentences, T_2 (T_6), (c) imperatives, T_3 (T_4?), and (d) exclamatory sentences, T_5 (T_6?). In short, any given transmitter, say for instance a declarative sentence, can be plausibly interpreted to express statements which might have been just as well or even more precisely expressed by, say, interrogative or imperative, or exclamatory sentences. Or in other words, the general skepticism toward language as a means to render the inter-and intrapersonal communication more effective on one side, and sharpened sensitivity for language ambiguities on the other, join forces in preventing the hermeneutical possibilist from believing in any distinction

between statements (or concepts) made on grounds of grammatical categories or similar differences in the construction or in other outward shapes and skeletal forms of the transmitters employed. Thus, regardless of what we intend to understand by "normative statements" and by "descriptive statements," the possibilist view would be that any conceivable sentence, T_0, may be interpreted to express an indefinite number of both normative and descriptive statements.

It goes, I hope, without saying, that what above is said about normative and descriptive statements also holds true for the remaining distinctions mentioned: analytic/synthetic, logically odd/logically orthodox, meaningful/meaningless . . .

As part of a rather extensive study[1,2] of 147 students, who up to then had been relatively uninfected with logic or semantics in any form, 75, Group I, were exposed to a short lecture (207 words), phrased with the purpose to elicit what may be described as a "*logico-maniacal*" attitude, and 72, Group II, to another lecture (255 words) with a more "*common sensical*" bias.[13] Both groups were confronted with a questionnaire consisting of 31 sentences,[14] and given the task to pick out a) tautologies b) contradictions and c) nonsensical sentences. Group I intuited the classifications without restraint. Not more than 10 per cent of this group found in as many as eight cases any difficulties at all in classifying the sentence concerned in one of the three categories. In equally as many cases was the decision unanimous. The following sentences were "*seen*" by 73 or more out of 75 to be either (a) tautologies[15] (b) self-contradictions or (c) nonsense sentences.

(a) *Tautologies:* statements no.s 1. Boys will be boys. 2. A dollar is a dollar. 3. What is done, is done. 6. When one says a thing that is not true, one lies. 9. When one is bedridden, one stays in bed. 12. this sentence is sentence No. 12 in the present questionnaire. 13. When it rains in Berkeley, it rains in Berkeley. 17. A spinster has no child. 18. A guy is a guy. 19. Nothing is both round and square. 21. Deaf cannot hear. 22. Here in the West, men are men. 23. Behind the clouds the sky is always blue. 24. Rose is a rose is a rose is a rose. 27. The Berkeley campus of University of California is in Berkeley, California. 29. I see what I see.

(b) *Self-contradictions:* statements no.s 11. Not all unmarried men are bachelors. 16. The coffee in the coffeeshop is not coffee. 20. The absent-minded professor was looking for the book, even though he knew where it was. 28. Nothing is more exciting than a commonplace platitude. 30. Kim Novak is pretty, and at the same time she is not.

31(b) Today is Thursday, and I don't believe it. (Or, in 15 question-naires:) 31(a). Ron Currie has left the room, and I don't believe it.

(c) *Nonsense sentences:* statements no.s 4. The stuffed lungs of the fox twitch and cry love, and the strutting fern lay seeds on the black sill. 7. The holy ghost in Meadowsweet Hall, Higglefore-cum-wortlebury-beneath-the-Hill was only an anemic yawn of a dandruffed hippopotamus. 10. Either the nothing exists only because the not, i.e. negation exists, or negation and the not exist only because the nothing exists.

This nearly perfect harmony within Group I is only marred by six sentences which were classified just as unmistakably as the others, but differently by a material number of different respondents. Only one sentence, however, was to an equal extent classified in all three categories, viz. No. 25. Unicorns are extinct.

(b) Group II, on the other hand, the group with a "common sense bias," seemed almost equally capable of intrepreting all 31 sentences to transmit plausible, reasonable, fairly interesting, more or less tenable hypotheses or more or less advisable proposals. Only two sentences, both classified as "tautologies" of Group I, seemed to cause hermeneutical difficulties. Nine students resigned *vis-à-vis* sentence No. 12, twelve *vis-à-vis* No. 27; they were both bound to be tautologies! And characteristically, neither of these two sentences would ever have been recognized as tautological or even as analytic by any presumably competent logician. The other sentences, however, of which some are traditionally (and, of course, by our Group I) given as examples of (self-)contradictions or at least of "logical oddities" were easily and unanimously (or practically unanimously) interpreted to express somewhat controversial synthetic statements.

Among sentences the (only) plausible interpretation of which did not seem to raise a shadow of doubt in any members of Group II, a few deserve to be mentioned. Indicated below are some of the most frequent directions of interpretation (precization) in Group II:

No. 11: Not all unmarried man lead a free ... wild ... unconventional ... bohemian life; some have gone steady with the same girl for years, are engaged to be married ... etc.

No. 16: The coffee ... is (excessively) weak.

No. 30: K. N. is pretty only in some respects ... from certain angles ... sometimes ("at the same time" is apparently interpreted as synonymous to "yet" ... "still ..." "on the other hand").

No. 31a: I can hardly believe that R. C. had the nerve ... would have dared to ... leave the room.

No. 31b: I can hardly believe that it is (already) Thursday.

The last two examples are particularly interesting: They belong to a type of sentences ("X is true and I don't believe it." — i.e. that x is true) quite commonly used to exemplify "logical oddities" or even "contradictory sentences." And it seems difficult, even to a hermeneutical possibilist, to think of *plausible* or "logical" or "literal" interpretations of 31a and 31b fit to illustrate anything in the direction of *logical* oddness. Everybody seems familiar with situations where something was true which he at that time did not believe to be true.[16] There is hardly anything odd to be found in the following two reports: (b) "Yesterday was Thursday and I did not believe it," (a) "I did not believe that R. C. had left the room when he (actually) had left it." Quite another thing is the fact that it probably would require a Marcel Proust to seize such situations while they were still present, and momentarily report them in an adequate tense. It is this *practical-psychological* queerness which have caused our "common sense" students to discard any such "logical" or "literal" interpretations of the sentences 31b and 31a in favour of the previously indicated, more plausible i.e. more frequently occurring interpretations. In other words: The two mentioned sentences may more or less plausibly be interpreted to convey statements which it very rarely would be necessary to transmit. And yet it is imperative that, whenever this necessity occurs, the adequate linguistic means are at hand. The prevalent confusion of 'logical oddity' and 'infrequent occurrence' may — because of logical oddities' impermissibility — effectively prevent uncommon, unusual, extraordinary sentences from being employed, even in the rare cases when effective verbal communication would entirely depend upon their availability. This is why it is so important to maintain a certain *tolerance towards locutional curiosities*. And this is why (for communication purposes) it would be safer to assume that in general all so-called "logical oddities" are virtually "infrequent occurrences."

Group II was also given the task to predict how "these hair splitting logicians" would classify the same 31 sentences. Their predictions of Group I's classifications were almost perfect (ratio 10.8/11.3). There was only one major exception: a sentence which was "seen" to be "self-contradictory" by almost 90 per cent (63) of Group I and which absolutely *none* in Group II would suspect even "hair-splitting logicians" of "seeing" or conceiving of as anything but a hackneyed truism about professors. The sentence (No. 20) reads: "The absent-minded professor was looking for the book, even though he knew where it was."

Members of Group I would apparently reject this sentence because they interpreted it to transmit a negative analytic ("self-contradictory") statement. Group II, on the other hand, was unanimously willing to accept the same sentence as expressing a positive synthetic statement. In other words: It would serve as an effective illustration of what Arne Naess calls "pseudo-disagreement."[17]

In order to bring out this point a little more extensively, the above study was, after three weeks, followed up by another, where 25 of the 31 sentences which lent themselves most readily to a true/false classification, were given to the same sample of students (Group I and Group II) with the following instructions:

"1. Read carefully the 25 sentences listed below. Try to decide for each one of them whether you think it can express: (a) a true or (b) a false statement."

"2. State your reasons for each (true or false) decision."[18]

The collected material furnished a score of splendid examples of pseudo-disagreements between the two groups, plus a few useful examples of pseudo-agreement, all apparently due to overoptimism on the part of the respondents regarding their ability to "*see*" *what can possibly be meant by a sentence* combined with insufficient sensitivity to and awareness of language ambiguities.

IV. Hermeneutical Obtuseness and "What Should We Say?"

In conclusion we may repeat: The present article endeavors *inter alia* to furnish arguments *for* a Humpty-Dumpty attitude to language, and against any tendencies to narrow down the field of permissible communication by employing rigid, *a priori* norms or rules for "what can possibly be said and meant."

Particularly misleading is the procedure where the linguistic legislator arrives at his "laws" by asking questions of the skeletal form: "is it possible to x?" and conclude from a more or less unanimous "no" to such questions as whether one can (not) ever say and mean: "P did x." Most people would probably, off hand, deny that it is possible to admire a landscape with closed eyes. But this would not prevent them from imagining situations — odd or not — where the necessity *might* occur for expressing a statement which would be most adequately transmitted, for instance, by the stentence: "P admired the landscape with closed eyes." The whole thing is most often a question of general (including

hermeneutical) imagination. We may even, if necessary, equip Mr. P with transparent eyelids, or turn him into an eidetic (with positive after-images) etc. . . . Fifty years ago nobody would believe it possible for a person to continue his (biological) life after the heart had stopped beating. In fact, the following sentence, T, "His heart stopped beating and he did not die" might have been commonly considered a "self-contradiction," "a logical oddity," "a sin against language" and what have you. Today things are taking place in operating rooms all over the world which would be most adequately described by means of sentences similar to T above.[19] Moreover, there are distinct tendencies within the more developed sciences to formulate sentences and symbols intended to transmit statements and concepts more or less *unimaginable*, at least to the present generation. The megalomaniacal armchair philosopher who will (*a priori*) establish *today* what can possibly be said and meant and done *tomorrow*, reminds me of a highly esteemed Danish writer who, at the turn of the century, argued as follows in an encyclopedia article on "Flying Machines." "It is obvious," he concluded, "that none of these fantastic ideas will ever be realized. Since, as everybody knows, nothing heavier than water can float in water, nothing heavier than air can ever fly in the air." And while he was writing, birds sailed through the sky . . .

In other words, our problem is not: "What *can* we say," but "what *should* we say." The remaining part of the present article is devoted to a brief summary of a few studies intended to throw light on some important communicational difficulties in connection with the transmitter, T_0, *Should we ever say* . . .

The introductory step of the study was an assignment done by 72 semantically trained students. They constructed the list below of presumably important, plausible and cognitively different precizations of T_0.

T_0 Should we ever say . . .

T_1 *Would* we ever say . . .

T_{11} Would we *possibly* ever say . . .

T_{111} Is it possible that we under any circumstances would *happen* to utter . . .

T_{112} Is it possible that we under any circumstances would *want* to utter . . .

T_{12} Is it likely that we would ever say . . .

T_{121} Is it likely that we would ever *happen* to utter . . .

T_{122} Is it likely that we would ever *want* to utter . . .

T_2 Would it ever be *permissible* for us to say . . .

T_{21} Is it *possible* that it would ever be permissible for us to say ...

T_{211} Is it possible that under hitherto unknown and unforeseen circumstances it would, according to *present standards,* be permissible to say ...

T_{212} Is it possible that we, due to a *change* in our present standards, would ever come to *regard* it as permissible to say ...

T_{22} Is it *likely* that it would ever be permissible for us to say ...

T_{221} Is it likely that, according to *present standards,* it would ever be permissible for us to say ...

T_{222} Is it likely that there would be a *change* in our present *standards* so that we would come to *regard* it as permissible to say ...

T_{23} Is it (ever) idiomatic to say ... (cf. T_{12}!)

T_{24} Is it (ever) grammatically or syntactically correct to say ...

T_{25} Would we (ever) transmit a true statement when we say ...

T_{251} Is it likely, given the present circumstances, that we would (ever) transmit a true statement when we say ...

T_{252} Is it possible that we, under any conceivable circumstances, might be transmitting a fairly tenable statement when we say ...

T_{26} Would we (ever) transmit a significant (worthwhile) statement when we say ...

T_3 Would it ever be advisable for us to say ...

This list of precizations of T_0 represents in itself a warning against any attempt to take verbal *agreement* on T_0 as a symptom of *actual* agreement on an issue, a subject matter — or *disagreement* on T_0 to indicate *actual dis*agreement. As long as two or more of the main directions of precization of T_0 are slurred over, the chances are that the verbal agreement will turn out to be a pseudo-agreement, the verbal disagreement to be a pseudo-disagreement.[17]

Pretests seemed to promise that the most spectacular pseudo-disagreements would arise due to hermeneutical obtuseness *vis-à-vis* an ambiguity consisting of a T_{23} and a T_{24} direction of precization on one hand and the T_{25} direction of precization on the other. Consequently we constructed two questionnaires: 1. "*Language* Qst." and 2. "*Tenability* Qst." Both presented eighteen "Should we ever say ..." questions to the 198 adult respondents (non-students) who were asked to answer either "yes" or "no" to the questions and state their *reasons why we should say ...,* and *why we should not say ...* respectively. The last fifteen questions were the same in both questionnaires, whereas the first three were different.[20] The *language* questionnaire (Qst. 1) started out with a set of sentences where a decision as to whether "we should say" such

sentences or not obviously had to do with their *correctness* from a grammatical-syntactical or idiomatic point of view. The *tenability* questionnaire (Qst. 2) had a corresponding introductory set of three sentences, the acceptability of which was just as clearly dependent upon whether they seemed to transmit tenable *hypotheses* or not.

The remaining fifteen questions did not lend themselves too unambiguously to either of the above interpretations. We therefore ventured to guess that: a) Qst. 1. — respondents would be apt to let *their* standpoint ("we should say" or "we should not say") to each one of the next sentences depend upon grammatical-syntactical correctness and idiom, and; b) Qst. 2. respondents would hold that whether "we should say" a sentence or "we should not say" a sentence was solely a function of the tenability of the statement presumably transmitted by the sentence in question. In other words, we would have reasons to *suspect*[21] pseudo-disagreements between the two groups of respondents as well as generally between a) respondents who answered "yes" (respectively "yes") for tenability reasons. *Vice versa* for possible pseudo-*agreements* within groups with the same answers (either "yes" or "no") to the last fifteen questions.

The results of our investigations, as illustrated by the below listed cross runs, seemed convincingly to support these hypotheses.[22]

			Language reasons	Tenability reasons	Language & Tenability		no reason	Total
					Both	Neither		
Should we ever say: No. IV:	*Language* *Qstn.*	yes	6	16	0	0	2	24
		no	45	11	10	3	0	69
"A comatose (i.e. completely unconscious) girl in Pennsylvania knitted a perfect sweater for her brother, absolutely unintentionally"?		total	51	27	10	3	2	93
	Tenability *Qstn.*	yes	4	34	2		1	41
		no	5	40	0		0	45
		total	9	74	2		1	86
No. V:	*Language* *Qstn.*	yes	4	9	3		6	22
		no	46	11	5		2	64
"Judas was actually assassinated, he did not intend to commit suicide"?		total	50	20	8		8	68
	Tenability *Qstn.*	yes	4	22	3		0	29
		no	5	37	9		1	52
		total	9	59	12		1	81

Should we ever say:

			Language reasons	Tenability reasons	Language & Tenability Both	Language & Tenability Neither	no reason	Total
No. VI: "Francis Bacon did not plan to write Shakespeare's Hamlet, he dashed it off more or less inadvertently"?	Language Qstn.	yes	3	4	4		1	12
		no	41	25	10		2	78
		total	44	29	14		3	90
	Tenability Qstn.	yes	4	14	0		1	19
		no	2	52	7		1	62
		total	6	66	7		2	81
No. VII: "Most people swear and curse unintentionally, merely from force of habit, but some do it intentionally, and some even on purpose"?	Language Qstn.	yes	2	36	8			46
		no	43	4	0			47
		total	45	40	8			93
	Tenability Qstn.	yes	19	32	0		1	52
		no	7	20	6		2	35
		total	26	52	6		3	87
No. IX: "If we are going to do a thing, we might just as well get it over with"?	Language Qstn.	yes	8	31	6		3	48
		no	30	5	7		0	42
		total	38	36	13		3	90
	Qstn. Tenability	yes	8	32	9		1	51
		no	4	28	2		1	35
		total	12	60	11		2	86
No. X: "It is bedtime, I am alone, I yawn: but I do not yawn involuntarily (or voluntarily!) nor yet deliberately. To yawn in any such peculiar way is just not to just yawn"?	Language Qstn.	yes	5	14	3		1	23
		no	48	9	10		1	68
		total	53	23	12		2	91
	Tenability Qstn.	yes	19	4	0		2	25
		no	5	31	22		0	58
		total	24	35	22		2	83
No. XII: "Everything that Einstein wrote down in his later years has been saved for posterity, even his casual and unintentional doodlings"?	Language Qstn.	yes	10	32	2	2	0	46
		no	38	5	0	0	2	45
		total	48	37	2	2	2	91
	Tenability Qstn.	yes	2	20	5		2	29
		no	1	51	4		0	56
		total	3	71	9		2	85

			Language reasons	Tenability reasons	Language & Tenability		no reason	Total
					Both	Neither		
No. XIII:	Language Qstn.	yes	23	17			4	44
		no	8	38			0	46
		total	31	55			4	90
"Everything may actually be green"?	Tenability Qstn.	yes	4	31	6			41
		no	2	36	4			42
		total	6	67	10			83
No. XIV:	Language Qstn.	yes	0	15	2		1	18
		no	27	35	9		1	72
		total	27	50	11		2	90
"Everything may actually be horse"?	Tenability Qstn.	yes	9	3				12
		no	15	59				74
		total	24	62				86
No. XV:	Language Qstn.	yes	2	37	5			44
		no	38	8	1			47
"There is a form of mental insanity where the victims – quite unintentionally of course – are perpetually tying strings across the top of staircases"?		total	40	45	6			91
	Tenability Qstn.	yes	3	31	4		4	42
		no	2	38	0		1	41
		total	5	69	4		5	83
No. XVI:	Language Qstn.	yes	3	44	1		4	52
		no	22	15	0		0	37
		total	25	59	1		4	89
"Everything may actually be a dream or a hallucination"?	Tenability Qstn.	yes	6	31	4		5	46
		no	3	29	1		0	33
		total	9	60	5		5	79
No. XVII:	Language Qstn.	yes	22	10	1		1	34
		no	52	5	1		0	58
		total	74	15	2		1	92
"The Titanic catastrophe was a mere accident"?	Tenability Qstn.	yes	24	7	6		1	38
		no	10	31	0		0	42
		total	34	38	6		1	80

Similar if not more striking results might undoubtedly have been obtained by using as our point of departure any other pair of the student's twenty-one cognitively different precizations of "should we ever say." Anyway, it seems advisable to be *en garde* with sharpened sensitivity and awareness of language ambiguities, whenever key-sentences like "should we ever say," "what should we say when" etc. are introduced in seminar discussions on language usages. As long as participants fail to make explicit which of the major directions of precization they have in mind (want to transmit) when they employ such sentences, so long will we have reasons to suspect that any *agreement* reached on "what we should say when," would easily be revealed as a *pseudo*-agreement.[23]

The significance of this point is quite adequately brought out in a set of empirio-semantical investigations of the question "when should we ever point to an action, x, and say: That action (x) is *voluntary, involuntary,* or *not voluntary?* (respectively)?[1,4]

The most conspicuous hermeneutical discrepancies were here found between respondents who would tend to be concerned with the *advisability of adopting a certain terminology* (from different moralistic, hedonistic, humanitarian, socio-psychological . . . points of view), and respondents only discussing the *likelihood* of anybody (ever) uttering: "X is voluntary" etc. under the indicated circumstances. The most interesting difficulties, however, did not arise until this normative — versus prediction — direction of precization was well overcome. Then, it appeared that some respondents understood the sentence "*would* you (ever) say that x was voluntary?" to mean something like: "Say you were given the choice to classify x either under the heading of 'voluntary actions' or under 'involuntary actions' (or 'not voluntary actions'), what would you choose?" Others, apparently, thought the following was what we were asking them: "Is it a tenable hypothesis about (or description of) your behaviour to predict that under the given circumstances you would point to the action x and utter: "X is voluntary (involuntary, not voluntary —)"?". While the first group was trying to solve subsumability problems, the latter "searcheth their reins and hearts" to find out how they would (probably) act . . . None the less quite often the two groups would come out with the same decision, *e.g.* "I would say . . ." or "I would *not* say . . . ." Analyses of agreements on negative answers proved particularly revealing and rewarding. The first group would *not* say: "X is voluntary," *because of practical or theoretical difficulties in determining where on the voluntary-involuntary continuum the action*

in question (x) should be located. The second group would *not* say: "X is voluntary", *because the classification of x as a voluntary action was too obvious.* And who does ever want to say the obvious? Thus, in a standard situation, nobody would ever point to a person who goes to enjoy a good show and say: "That person there is performing a voluntary action." One might just as well have said: "That person is a person." To say about an action which is obviously voluntary, that it is a voluntary action, is trivial, redundant. One would never do so except under very special circumstances, e.g. when teaching children or foreigners to speak English, and the like (see below).

It must be this well-known phenomenon which has led some philosophers[24] to believe that we would (should? can?) only say "x is voluntary" provided something seems fishy about x.

A study on "Trivial and Worthwhile Hypotheses"[1.15] reveals convincingly this universal reluctance to express the obvious (without a good pretext). We would never under ordinary circumstances point to an ordinary chair, and ordinary table, an ordinary man and say: "that is a chair", "that is a table", or "that is a man", unless there was something fishy about the chair, the table, the man . . .

"It goes without saying that a hypothesis found to be invariably *non*-controversial within a certain reference of communication, R_a, will rarely be propounded within R_a except in order to form a basis or a platform on which R_a-communicators can stand and debate the relative tenability of other, genuinely controversial hypotheses within R_a ."[25]

Thus, if we are discussing an object which seems to be on the border-line between a chair and a stool, *i.e.* a "fishy" chair, it might be a good idea to start out with an agreement on a more trivial, less controversial hypothesis by pointing to an ordinary chair and saying: "Do you agree that this thing here is a chair? OK? Then I will try to show to you that: *If this* is a chair, *then* that (not so subsumable) object is a chair." The same goes for voluntary actions. We may try to solve the problem whether x is a voluntary action, by pointing to an obviously voluntary action, y (e.g. to go to enjoy a good movie) and show that accepting y as a 'voluntary action' argues *for* (or against) accepting the more controversial ("fishy") x as a voluntary action. Thus there *are* cases where it is necessary to call a spade "a spade" and a voluntary action "a voluntary action."

The last five questions of the questionnaire (in "The Worthwhile Hypothesis Study"[1.15]) read as follows:

(11) Should we ordinarily say: "Today I met a man who was 5'10" tall?"
Why? Why not? (Explain)

(12) Should we ordinarily say: "Today I met a man who was 6'6" tall?"
Why? Why not? (Explain)

(13) Should we ordinarily say: "Today I met a man who was 8'1" tall?"
Why? Why not? (Explain)

(14) Should we ordinarily say: "Today I met a man who was 9'9" tall?"
Why? Why not? (Explain)

(15) Should we ordinarily say: "Today I met a man who was 15' tall?"
Why? Why not? (Explain)

The distribution of "yes" and "no" answers to these questions was illustrative:

	11 (5'10")	12 (6'6")	13 (8'1")	14 (9'9")	15 (15')
yes	91	303	511	128	12
no	453	246	38	109	520
other	14	9	9	21	26

Question no. 13. gives the ideal amount of audacity and (probably) tenability to be *worthwhile saying*. 14. and (especially) 15., however, are apparently not conceived of as *tenable*. 11. is considered trivial. 12. is a borderline case.

To sum up

We have frequently stated our verbal preference for the question: "What *should* we say?" as compared to "What *can* we say?"; but only if interpreted in the direction of T_3: *What is advisable* (for us) *to say* (when)? Any universal and categorical answer to this question is bound to be either misleading or trivial. It all depends upon the purposes we have in mind, general and specific, when communicating with ourselves or our fellow beings. And our current purposes and long term goals are again dependent upon an adequate inter- and intra-personal communication. "Thrown out in a world where anything is possible," as

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some existentialists would say, we should always be prepared to make use of linguistic expressions, which, off hand, may sound odd, extraordinary, or preposterous, in order to transmit statements which are odd, extraordinary, preposterous. It is imperative to have free hands in "making words mean what we choose them to mean" as Humpty Dumpty said — "neither more nor less." And are the words at any time recalcitrant, then we will show them who is to be the master — that's all.

NOTES

[1] The investigations were carried out in the Speech Dept. of the University of Calif. at Berkeley, mainly in my 198 study group ("Verbal and argumentational preferences as revealed by empirio-semantical investigations") and in my 162 seminar ("Theory of interpretation"). Students are responsible for the interviews, the statistics and in some cases for a preliminary report. Of studies pertinent to the present article, the following fifteen deserve to be mentioned:

[1.1] *"Should we ever say . . ."* Statistics and report by *R. Rutherford.* (A random sample of 198 adults in Berkeley were confronted with two 3 hour questionnaires).

[1.2] *The Analytic/Synthetic Distinction.* Statistics and report by *Th. Stern.* (Three 1–3 hour questionnaires. 147 students interviewed plus 279 in two short pretests).

[1.3] *Definiteness of Intention* – *M. Grizzle* (72 students).

[1.4] *The Voluntary/Involuntary Distinction* – *Page Anderson.* (402 adults selected at random and 72 students) Four questionnaires.

[1.5] *"P can decide to x."* – *James Small.* (72 Students).

[1.6] *"That is no excuse"* (1,260 adults interviewed by 63 students. Individual reports by the students).

[1.7] *"What can we say?"* (Linguistic rigidity and verbal preferences) I. – *D. Cox* and *R. Oliver.* (120 children from 4–12 years, informal interviews and three types of questionnaires).

[1.8] *"What can we say?"* II – *R. Rutherford, B. Hartmann* and *D. v. Bremen* (Three questionnaires to 570 fourth, fifth, and sixth graders in 18 classes in 9 Berkeley San Francisco Grammar Schools).

[1.9] *"What can we say?"* III – *N. Knight* and *Ph. Maier* (Three questionnaires to 96 High School students and 63 adults – sixty years old or older).

[1.10] *"What can we say?"* IV – (Three questionnaires distributed in class to 93 undergraduate university students.)

[1.11] *"What can we say?"* V – *W. Stricklin* (Two types of questionnaires to 202 students at the California State School for the Deaf, Berkeley.)

[1.12] *Linguistic Rigidity and Verbal and Argumentational Preferences in Religious Reasoning.* – *D. Werronen* (3–4 hour questionnaire to 207 representatives – students and others – of different religious groups in Berkeley, Oakland, and San Francisco).

[1.13] *Linguistic Rigidity and Verbal and Argumentational Preferences in Political Discourse.* – *S. Sardell* (5 types of questionnaires to 57 members of The League of Women Voters in Los Angeles, Santa Monica, Berkeley, and Oakland).

1.14 "mere" and "merely." (Questionnaire filled in by 93 undergraduates and 279 non-students).

1.15 *Trivial and Worthwhile Hypotheses.* (Questionnaire filled in by 93 students and 465 non-students. This study is a "follow-up" on 1.1 and 1.2 above).

These fifteen projects, among others, will be dealt with in greater detail in a forthcoming work: *Controversies.* (Factual and Fictitious Controversies in the Light of Empirio-semantical Investigations). However, I do not want to miss this opportunity of thanking my 162, 198, and 199 students for their amazing enthusiasm and splendid achievements. I also want to express my heartfelt gratitude to: Prof. *John Austin,* whose inspiring lectures, seminars and more informal discussions have provoked most of the above listed projects (1.1, 1.4, 1.6, 1.14, and 1.15 in particular), Professor *Isabelle Hungerland,* Professor *Lewis Feuer,* Professor *Benson Mates,* Professor *Willard Quine,* and Professor *David Rynin* for stimulating arguments and conversations and for important comments and suggestions.

2 1782–1846 Cf. Frederik Böök: "Den Romantiske Tidsålderen" (in: O. Sylwan's Svenska Litteraturens Historia II, Stockholm 1929.)

3 Novum Organum I, 43.

4 *i.e.* transmitters which appear: incorrect, ungrammatical, unsyntactical, unidiomatic, illogical, impermissible, not in place, unlucky, injudicious, invidious, trivial, redundant, self-evident, analytic, tautological, obviously true, obviously false, contradictory, non-sensical, meaningless, preposterous . . .

5 Such as: true/false, good/evil, analytic/synthetic (instructive), a priori/a posteriori, real/imagined, voluntary/involuntary (not voluntary), table/desk (not table), chair/stool (not chair) . . . etc.

6 D. Krech and R. S. Crutchfield: *Elements of Psychology,* p. 463.

7 L. S. Vigotsky "Thought and Speech." *Psychiatry,* Vol. 2, February, 1939, No. 1, page 36.

8 Cf. my article "On Worthwhile Hypotheses," *Inquiry Vol. II,* No. 1.: "Nobody would ordinarily *want* to transmit to himself or any other receiver, a hypothesis of an extreme degree of triviality. Consequently, a suggestion for precization of a sentence . . . in the direction of an extreme degree of triviality, should, by and large, be rejected as *implausible.* And it is the more implausible the more conspicuously trivial the transmitter seems to be."

9 At this point, the playground instructor made a pause and chatted a little with the child to make sure that the story was heard and understood. All the mystery-mongering ("You must promise not to tell anybody . . ." etc.) plus the fact that the child was alone in the instructor's cabin with three grown-ups and a microphone, seemed in some cases to lessen the child's power of concentration.

10 Needless to say, all the 54 respondents in the "permissibility" (T_2) directed group answered "no" to all the "can you call . . ." questions.

11 They may well be attached to the three characteristics of 'hermeneutical possibilism': 'Logical clairvoyance', has to do with '*lack* of general skepticism towards language.' 'Hermeneutical obtuseness' is the same as '*undeveloped* sensivity to language ambiguities'; and 'intolerance of locutional curiosities' speaks for itself and will not be dealt with to any greater extent here.

12 Oslo 1955, p. 6 f. Cf. also "The fight against revelation in semantical studies." *Synthese* Vol. VIII (1950–51). Issue 2, Nos. 3–5 Pp. 225–235.

Below are the two "lectures":

I (Logico maniacal)

Most people express themselves very inaccurately and illogically. Often they do not even realize what they are actually saying. One can hear people say, for instance: "You couldn't possibly tell me where I can find the men's room?" They do not understand that, in an attempt to be polite, they are in fact accusing the other person of not being able to tell where the lavatory is located.

We will here consider three ways in which people can be illogical:

a) Consider the following familiar cliché: "A guy is a guy." This is, of course, a very silly thing to say. Naturally a guy is a guy, what else could he possibly be? It is in fact like saying "A = A", "2 = 2" and so forth. We will call such sentences: "*tautologies.*"

b) Today I heard our neighbour screaming: "Somebody has dropped paint on my car. It is green and red all over." Of course, no single object can at the same time be both red and green all over. We will say that my neighbour on this occasion expressed a *self-contradiction.*

c) Another phrase one quite often hears is this: "The mail is closed." Of course, mail is not a thing that can be closed. Such a sentence we call: *nonsensical.*

Please try to pick out a) tautologies, b) contradictions, and c) nonsensical sentences in the following list of sentences. Put an *a* in the margin for *tautologies;* a *b* for *contradictions;* and a *c* for *nonsensical sentences. State on a separate sheet the reasons for your judgment* (including, of course, cases where you find that the sentence is *neither* a tautology *nor* a self-contradiction *nor* nonsensical, but simply expressing an ordinary, maybe somewhat controversial statement.).

II (Common sensical)

It is good to try to be precise and "logical." But even more important is it to be able to *understand* what our fellow beings attempt to convey to us – particularly when the sentences they use sound a bit imprecise and illogical. What we need then is not an attitude of "logical hairsplitting," but more patience and tolerance and knowledge about our language and how it is actually used in our day by day dealings with it.

Thus we know that when a person says to us: "You couldn't possibly tell me where I can find the men's room," he does not mean to accuse us of not being able to tell him where to find it! Likewise when we say: "A guy is a guy" we never intend to convey a superfluous platitude like "A = A" or anything of that order. Or as we will say: *Nobody ever wants or intends to express a tautology.* The same is true for statements which are obviously false. E.g. if a person says: "My car is red and green all over", we can rest assured that he does *not* want to tell us that he has a car, the entire surface of which has the colour green; and at the same time the colour of this same surface is red! In short: *Nobody ever wants or intends to express a self-contradiction.*

Finally, it goes without saying that: *Nobody ever wants or intends to express a completely nonsensical statement.*

Please read down the following list of sentences carefully. Whenever you come to a sentence which you think these rigid, "hairsplitting logicians" would call "a *tautology*",

put an (*a*) in the margin; put a (*b*) if you "see" an alleged "*self-contradiction*"; and a (*c*) if you find sentences that some "high-brows" might feel tempted to call "*nonsensical*".

Write on a separate sheet your own, plausible interpretation of the different sentences, and give reasons *why* you think one, some, or all the sentences are *neither* tautologies, *nor* self-contradictions, *nor* nonsensical. NB! *If this is what you think!* In cases where you do not think that a sentence should be classified outside the three categories, (a), (b), (c), give reasons why you think so.

[14] Most of which were taken from a pretest of 279 freshman students, whose 48 sentences were to be classified in one of following six categories:
1. Positive analytic ($A = A$).
2. Negative analytic ($A = $ nonA).
3. Positive synthetic (Most human beings have one head.).
4. Negative synthetic (Nobody has a head.).
5. Controversial synthetic (Martians exist.).
6. Nonsense (The square of a chancellor walnut shell equals the third root of nightmare over sixty-six.).

The result showed a distinct tendency to logico-mania ('analytic' answers make up more than $2/3$ of total).

Category	1 Pos. anal.	2. Neg. anal.	3 Pos. syn.	4 Neg. syn.	5 Controversial	6 Nonsensical	Maximum of possible scores
Scorer	3521	3085	2247	1116	1083	2340	13392

The corresponding results from 93 students with some training in empirical semantics.

1 Pos. anal.	2 Neg. anal.	3 Pos. syn.	4 Neg. syn.	5 Controversial	6 Nonsensical	Sum of Scores
11	2	1596	1204	1634	18	4634

[15] "Tautology" is here used instead of the more unfamiliar "analytic," "positive analytic" or "analytically true sentence" (wherein included tautologies in the more technical sense of "tautology"). Likewise (in spite of its redundancy) we use "self-contradiction" for "negative analytic," "analytically false sentence."

[16] As once pointed out by Benson Mates.

[17] *Interpretation and Preciseness* Oslo, 1953, p. 125. And for *pseudo-agreement:* Chapter III 122–142.

[18] After having handed in this information the respondents were asked to look over the 25 sentences once again, and see if they would be able to give a further specification as to how true or false they found each one of the transmitted statements to be. These were the six categories:
1. The statement is bound to *be true under all conceivable circumstances.*
2. The statement is *obviously true but might be false* under very, very special circumstances.
3. The statement *may be true under some, and false under other circumstances.*

4. The statement is *obviously false,* but might be true under very, very special circumstances.
5. The statement is bound to be *false under all* conceivable circumstances.
6. It is *impossible to determine* how true or false the statement is; it all depends *upon how it is interpreted.*

The results:

Categories:	2	2	3	4	5	6	
Group I:	961	393	11	43	457	10	1875
Group II:	18	702	688	273	22	104	1800
Sum of scores:	979	1095	699	319	479	114	3675

[19] The same, of course, holds true for transmitter alternatives like "He was dead for so and so long, and today he is at fit as a fiddle" etc.

[20] These are the *Language* questions:
  I. Should we ever say: "There is no sea-sepents, because no one haven't never seen none?"
  II. Should we ever say: "The reason why Brutus stabbed Caesar, was because he simply purposed to?"
  III. Should we ever say: "The couple was married and went on its honeymoon?"
These are the *Tenability* questions:
  I. Should we ever say: "The interior of the earth is filled with grapefruit marmalade?"
  II. Should we ever say: "I met a man today who was more than 2,000 feet tall."
  III. Should we ever say: "In Sylacauga, Alabama, last year, Mrs. Ann Elizabeth Hodges, as she lay on the sofa in her living room, was hit in the stomach by a meteorite which came in via the roof, and sold it – the meteorite – for $2,700 to a Montana museum?"

[21] Here as elsewhere in this article the word "suspect" should be stressed. As long as we do not know the attitude of the *"language"* group to each question interpreted in the *"tenability"* direction of precization or the *"tenability"* standpoint to the same sentences when interpreted in the "language"-direction – so long will hypotheses concerning pseudo-agreements and pseudo-disagreements in our material be sheer guess-work. The most we could claim would be a *"mock-disagreement"* as this word is used in my article: "The System of Private Enterprise. An Empirio-Semantical Analysis of a Slogan," *Synthese* Vol. XI, No. 1, March, 1959 p. 78.

[22] The only exceptions were sentence No. VIII: "A poor excuse is no excuse" and sentence No. XI: "Sometimes people unintentionally utter French sentences." Both statements seemed controversial. But the whole sample (more than 93 %) was solely concerned with *tenability.*

[23] Vice versa for *dis*agreements, of course!

[24] Cf. *e.g.* Stanley Cavell: Must we mean what we say? *Inquiry*, No. 3, 1958, Vol. I.

[25] Cf. "On Worthwhile Hypotheses", *Inquiry* Vol. II. No. 2.